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THE HISTORY AND PRESENT STATE OF ELECTRICITY,

WITH

ORIGINAL EXPERIMENTS,

By JOSEPH PRIESTLEY, LL.D. F.R.S.

THE FOURTH EDITION,
CORRECTED AND ENLARGED.

Cauſa latet, vis eſt notiſſima. OVID.

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SERVANT,

JOSEPH PRIESTLEY.

THE

PREFACE

TO THE

FIRST EDITION.

IN writing the *Hiſtory and Preſent State of Electricity*, I flatter myſelf that I ſhall give pleaſure, as well to perſons who have a taſte for Natural Philoſophy in general, as to electricians in particular; and I hope the work will be of ſome advantage to the ſcience itſelf. Both theſe ends would certainly be anſwered in a conſiderable degree, were the execution at all anſwerable to the deſign.

THE Hiſtory of Electricity is a field full of pleaſing objects, according to all the genuine and univerſal principles of taſte, deduced from a knowledge of human nature. Scenes like theſe, in which we ſee a gradual riſe and progreſs in things, always exhibit a pleaſing ſpectacle to the human mind. Nature, in all her delightful walks, abounds with ſuch views, and they are in a more eſpecial manner connected with every thing that relates to human life and happineſs; things, in their own nature, the moſt intereſting to us. Hence it is, that the power of aſſociation has annexed crouds of pleaſing ſenſations to the contemplation of every object, in which this property is apparent.

THIS pleaſure, likewiſe, bears a conſiderable reſemblance to that of the ſublime, which is one of the moſt exquiſite of all thoſe that affect the human imagination. For an object in which we ſee a perpetual progreſs and improvement is, as it were, continually riſing in its magnitude; and moreover, when we ſee an actual increaſe, in a long period of time paſt, we cannot help forming an idea of an unlimited increaſe in futurity; which is a proſpect really boundleſs, and ſublime.

THE pleaſures ariſing from views exhibited in *civil*, *natural*, and *philoſophical* hiſtory, are, in certain reſpects, different from one another. Each has its advantages, and each its defects: and both their advantages and defects contribute to adapt them to different claſſes of readers.

CIVIL hiſtory preſents us with views of the ſtrongeſt paſſions and ſentiments of the human mind, into which every man can eaſily and perfectly enter, and with ſuch incidents, reſpecting happineſs and miſery, as we cannot help feeling, would alarm and affect us in a very ſenſible manner; and therefore, we are at preſent alarmed and affected by them to a conſiderable degree. Hence the pleaſure we receive from civil hiſtory ariſes, chiefly from the exerciſe it affords our paſſions. The imagination is only entertained with ſcenes which occaſionally ſtart up, like interludes, or epiſodes, in the great drama, to which we are principally attentive. We are preſented, indeed, with the proſpect of gradual improvement during the riſe of great empires; but, as we read on, we are obliged to contemplate the diſagreeable reverſe. And the hiſtory of moſt ſtates preſents nothing but a tedious uniformity, without any ſtriking events, to diverſify and embelliſh the proſpect. Beſides, if a man have any ſentiment of virtue and benevolence, he cannot help being ſhocked with a view of the vices and miſeries of mankind; which, though they be not all, are certainly the moſt glaring and ſtriking objects in the hiſtory of human affairs. An attention, indeed, to the conduct of divine Providence, which is ever bringing good out of evil, and gradually conducting things to a more perfect and glorious ſtate, tends to throw a more agreeable light on the more gloomy parts of hiſtory; but it requires great ſtrength of mind to comprehend thoſe views; and, after all, the feelings of the heart too often overpower the concluſions of the head.

Natural hiſtory exhibits a boundleſs variety of ſcenes, and yet infinitely analogous to one another. A naturaliſt has, conſequently, all the pleaſure which the contemplation of uniformity and variety can give the mind; and this is one of the moſt copious ſources of our intellectual pleaſures. He is likewiſe entertained with a proſpect of gradual improvement, while he ſees every object in nature riſing by due degrees to its maturity and perfection. And while new plants, new animals, and new foſſils are perpetually pouring in upon him, the moſt pleaſing views of the unbounded power, wiſdom, and goodneſs of God are conſtantly preſent to his mind. But he has no direct view of human ſentiments and human actions; which, by means of their endleſs aſſociations, greatly heighten and improve all the pleaſures of taſte.

The hiſtory of philoſophy enjoys, in ſome meaſure, the advantages both of civil and natural hiſtory, whereby it is relieved from what is moſt tedious and diſguſting in both. Philoſophy exhibits the powers of nature, diſcovered and directed by human art. It has, therefore, in ſome meaſure, the boundleſs variety with the amazing uniformity of the one, and likewiſe every thing that is pleaſing and intereſting in the other. And the idea of continual riſe and improvement is conſpicuous in the whole ſtudy, whether we be attentive to the part which nature, or that which men are acting in the great ſcene.

It is here that we ſee the human underſtanding to its greateſt advantage, graſping at the nobleſt objects, and increaſing its own powers, by acquiring to itſelf the powers of nature, and directing them to the accompliſhment of its own views; whereby the ſecurity, and happineſs of mankind are daily improved.

Human

Human abilities are chiefly conſpicuous in adapting means to ends, and in deducing one thing from another by the method of analogy; and where ſhall we find inſtances of greater ſagacity, than in philoſophers diverſifying the ſituations of things, in order to give them an opportunity of ſhowing their mutual relations, affections, and influences; deducing one truth and one diſcovery from another, and applying them all to the uſeful purpoſes of human life.

If the exertion of human abilities, which cannot but form a delightful ſpectacle for the human imagination, give us pleaſure, we enjoy it here in a higher degree than while we are contemplating the ſchemes of warriors, and the ſtratagems of their bloody art. Beſides, the object of philoſophical purſuits throws a pleaſing idea upon the ſcenes they exhibit; whereas a reflection upon the real objects and views of moſt ſtateſmen and conquerors cannot but take from the pleaſure, which the idea of their ſagacity, foreſight, and comprehenſion would otherwiſe give to the virtuous and benevolent mind. Laſtly, the inveſtigation of the powers of nature, like the ſtudy of Natural Hiſtory, is perpetually ſuggeſting to us views of the divine perfections and providence, which are both pleaſing to the imagination, and improving to the heart.

But though other kinds of hiſtory may, in ſome reſpects, vie with that of philoſophy, nothing that comes under the denomination of hiſtory can exhibit inſtances of ſo fine a riſe and improvement in things, as we ſee in the progreſs of the human mind, in philoſophical inveſtigations. To whatever height we have arrived in natural ſcience, our beginnings were very low, and our advances have been exceedingly gradual. And to look down from the eminence, and to ſee, and compare all thoſe gradual advances in the aſcent, cannot but give the greateſt pleaſure to thoſe who are ſeated on the eminence, and who feel all the advantages of their elevated ſituation. And conſidering that we ourſelves are, by no means, at the top of human ſcience; that the mountain ſtill aſcends beyond our ſight, and that we are, in fact, not much above the foot of it, a view of the manner in which the aſcent has been made, cannot but animate us in our attempts to advance ſtill higher, and ſuggeſt methods and expedients to aſſiſt us in our farther progreſs.

GREAT conquerors, we read, have been both animated, and alſo, in a great meaſure, formed by reading the exploits of former conquerors. Why may not the ſame effect be expected from the hiſtory of philoſophy to philoſophers? May not even more be expected in this caſe? The wars of many of thoſe conquerors, who received this advantage from hiſtory, had no proper connection with former wars: they were only analogous to them. Whereas the whole buſineſs of philoſophy, diverſified as it is, is but one; it being one and the ſame great ſcheme, that all philoſophers, of all ages and nations, have been conducting, from the beginning of the world; ſo that the work being the ſame, the labours of one are not only analogous to thoſe of of another, but in an immediate manner ſubſervient to them; and one philoſopher ſucceeds another in the ſame field; as one Roman proconſul ſucceeded another in carrying on the ſame war, and purſuing the ſame conqueſts, in the ſame country. In this caſe, an intimate knowledge of what has been done before us cannot but greatly facilitate our future progreſs, if it be not abſolutely neceſſary to it.

THESE hiſtories are evidently much more neceſſary in an advanced ſtate of ſcience, than in the infancy of it. At preſent philoſophical diſcoveries are ſo many, and the accounts of them are ſo diſperſed, that it is not in the power of any man to come at the knowledge of all that has been done, as a foundation for his own inquiries. And this circumſtance appears to me to have very much retarded the progreſs of diſcoveries.

Not that I think philoſophical diſcoveries are now at a ſtand. On the other hand, as quick advances ſeem to have been made of late years, as in any equal period of time paſt whatever. Nay, it appears to me, that the progreſs is really accelerated. But the increaſe of knowledge is like the increaſe of a city. The building of ſome of the firſt ſtreets makes a great figure, is much talked of, and known to every body; whereas the addition of, perhaps, twice as much building, after it has been ſwelled to a conſiderable ſize, is not ſo much as taken notice of, and may be really unknown to many of the inhabitants. If the additions which have been made to the buildings of the city of London, in any ſingle year of late, had been made two or three centuries ago, it could not have eſcaped the obſervation of hiſtorians; whereas, now, they are ſo ſcattered, and the proportion they bear to the whole city is ſo ſmall, that they are hardly noticed. For the ſame reaſon, the improvements that boys make at ſchool, or that young gentlemen make at an academy, or the univerſity, are more taken notice of than all the knowledge they acquire afterwards, though they continue their ſtudies with the ſame aſſiduity and ſucceſs.

The hiſtory of experimental philoſophy, in the manner in which it ought to be written, to be of much uſe, would be an immenſe work; but it were much to be wiſhed, that perſons who have leiſure, and ſufficient abilities, would undertake it in ſeparate parts. I have executed it, in the beſt manner I have been able, for that branch which has been my own favourite amuſement; and I ſhall think myſelf happy, if the attempt excite other perſons to do the like for theirs.

I cannot help thinking myſelf to have been peculiarly fortunate, in undertaking the hiſtory of electricity, at the moſt proper time for writing it, when the materials were neither too few, nor too many to make a hiſtory; and when they were ſo ſcattered, as to make the undertaking highly deſirable, and the work peculiarly uſeful to Engliſhmen.

I LIKEWISE think myſelf exceedingly happy in my ſubject itſelf. Few branches of Natural Philoſophy would, I think, make ſo good a ſubject for a hiſtory. Few can boaſt ſuch a number of diſcoveries, diſpoſed in ſo fine a ſeries, all compriſed in ſo ſhort a ſpace of time, and all ſo recent, the principal actors in the ſcene being ſtill living.

WITH ſeveral of theſe principal actors it has been my ſingular honour and happineſs to be acquainted; and it was their approbation of my plan, and their generous encouragement that induced me to undertake the work. With gratitude I acknowledge my obligations to Dr. Watſon, Dr. Franklin, and Mr. Canton, for the books, and other materials with which they have ſupplied me, and for the readineſs with which they have given me any information in their power to procure. In a more eſpecial manner am I obliged to Mr. Canton, for thoſe original communications of his, which will be found in this work, and which cannot fail to give a value to it, in the eſteem of all the lovers of electricity. My grateful acknowledgements are alſo due to the Rev. Dr. Price, F. R. S. and to the Rev. Mr. Holt, our profeſſor of Natural Philoſophy at Warrington, for the attention they have given to the work, and for the many important ſervices they have rendered me with reſpect to it.

To the gentlemen above mentioned the public is, likewiſe, indebted for whatever they may think of value in the *original experiments* which I have related of my own. It was from converſing with them that I was firſt led to entertain the thought of attempting any thing new in this way, and it was their example, and favourable attention to my experiments, that animated me in the purſuit of them. In ſhort, without them, neither my experiments, nor this work would have had any exiſtence.

THE hiſtorical part of this work, the reader, I hope will find to be full and circumſtantial, and at the ſame ſuccinct. Every new fact, or important circumſtance, I have noted as it aroſe; but I have abridged all long details, and have carefully avoided all digreſſions and repetitions. For this purpoſe, I have peruſed every original author, to which I could have recourſe; and every quotation in the margin points to the authority that I myſelf conſulted, and from which the account in the text was actually taken. Where I could not procure the original authors, I was obliged to quote them at ſecond hand, but the reference will always ſhow where that has been done. That I might not miſrepreſent any writer, I have generally given the reader his own words, or the plaineſt tranſlation I could make of them; and this I have done, not only in direct quotations, but where, by a change of perſon, I have made the language my own.

I MADE it a rule to myſelf, and I think I have conſtantly adhered to it, to take no notice of the miſtakes, miſapprehenſions, and altercations of electricians; except ſo far as, I apprehended, a knowledge of them might be uſeful to their ſucceſſors. All the diſputes which have no way contributed to the diſcovery of truth, I would gladly conſign to eternal oblivion. Did it depend upon me, it ſhould never be known to poſterity, that there had ever been any ſuch thing as envy, jealouſy, or cavilling among the admirers of my favourite ſtudy. I have, as far as my beſt judgment could direct me, been juſt to the merits of all perſons concerned. If any have made unjuſt claims, by arrogating to themſelves the diſcoveries of others, I have ſilently reſtored them to the right owner, and generally without ſo much as giving a hint that any injuſtice had ever been committed. If I have, in any caſe, given a hint, I hope it will be thought, by the offending parties themſelves, to be a very gentle one; and that it will be a *memento*, which will not be without its uſe.

I THINK I have kept clear of any mean partiality towards my own countrymen, and even my own acquaintance. If Engliſh authors are oftener quoted than foreign, it is becauſe they were more eaſily procured; and I have found a difficulty I could not have expected, in procuring foreign publications upon this ſubject.

I FIND it impoſſible to write a preface to this work, without diſcovering a little of the enthuſiaſm which I have contracted from an attention to it, by expreſſing my wiſhes, that more perſons, of a ſtudious and retired life, would admit this part of experimental philoſophy into their ſtudies. They would find it agreeably to diverſify a courſe of ſtudy, by mixing ſomething of action with ſpeculation, and giving ſome employment to the hands and arms, as well as to the head. Electrical experiments are, of all others, the cleaneſt, and the moſt elegant, that the compaſs of philoſophy exhibits. They are performed with the leaſt trouble, there is an amazing variety in them, they furniſh the moſt pleaſing and ſurpriſing appearances for the entertainment of one's friends, and the expence of inſtruments may well be ſupplied, by a proportionable deduction from the purchaſe of books, which are generally read and laid aſide, without yielding half the entertainment.

THE inſtruction we are able to get from books is, comparatively, ſoon exhauſted; but philoſophical inſtruments are an endleſs fund of knowledge. By philoſophical inſtruments, however, I do not here mean the globes, the orrery, and others, which are only the means which ingenious men have hit upon, to explain their own conceptions of things to others; and which, therefore, like books, have no uſes more extenſive than the views of human ingenuity; but ſuch as the air-pump, condenſing engine, pyrometer, &c. (with which electrical machines are to be ranked) and which exhibit the operations of nature, that is of the God of nature himſelf, which are infinitely various. By the help of theſe machines, we are able to put an endleſs variety of things into an endleſs variety of ſituations, while nature herſelf is the agent that ſhows the reſult. Hereby the laws of her action are obſerved, and the moſt important diſcoveries may be made; ſuch as thoſe who firſt contrived the inſtrument could have no idea of.

In electricity, in particular, there is the greateſt room to make new diſcoveries. It is a field but juſt opened, and requires no great ſtock of particular preparatory knowledge: ſo that any perſon who is tolerably well verſed in experimental philoſophy, may preſently be upon a level with the moſt experienced electricians. Nay, this hiſtory ſhows, that ſeveral raw adventurers have made themſelves as conſiderable, as ſome who have been, in other reſpects, the greateſt philoſophers. I need not tell my reader of how great weight this conſideration is, to induce him to provide himſelf with an electrical apparatus. The pleaſure ariſing from the moſt trifling diſcoveries of one's own, far exceeds what we receive from underſtanding the much more important diſcoveries of others; and a mere reader has no chance of finding new truths, in compariſon of him who now and then amuſes himſelf with philoſophical experiments.

Human happineſs depends chiefly upon having ſome object to purſue, and upon the vigour with which our faculties are exerted in the purſuit. And, certainly, we muſt be much more intereſted in purſuits wholly our own, than when we are merely following the track of others. Beſides, this pleaſure has reinforcements from a variety of ſources, which I ſhall not here undertake to trace; but which contribute to heighten the ſenſation, far beyond any thing elſe of this kind that can be experienced by a perſon of a ſpeculative turn of mind.

It is a great recommendation of the ſtudy of electricity, that it now appears to be, by no means, a ſmall object. The electric fluid is no local, or occaſional agent in the theatre of the world. Late diſcoveries ſhow that its preſence and effects are every where, and that it acts a principal part in the grandeſt and moſt intereſting ſcenes of nature. It is not, like magnetiſm, confined to one kind of bodies, but every thing we know is a conductor or non-conductor of electricity. Theſe are properties as eſſential and important as any they are poſſeſſed of, and can hardly fail to ſhow themſelves wherever the bodies are concerned.

Hitherto philoſophy has been chiefly converſant about the more ſenſible properties of bodies; electricity, together with chymiſtry, and the doctrine of light and colours, ſeems to be giving us an inlet into their internal ſtructure, on which all their ſenſible properties depend. By purſuing this new light, therefore, the bounds of natural ſcience may poſſibly be extended, beyond what we can now form an idea of. New worlds may open to our view, and the glory of the great Sir Iſaac Newton himſelf, and all his contemporaries, be eclipſed, by a new ſet of philoſophers, in quite a new field of ſpeculation. Could that great man reviſit the earth, and view the experiments of the preſent race of electricians, he would be no leſs amazed than Roger Bacon, or Sir Francis, would have been at his. The electric ſhock itſelf, if it be conſidered attentively, will appear almoſt as ſurpriſing as any diſcovery that he made; and the man who could have made that diſcovery, by any reaſoning *a priori*, would have been reckoned a moſt extraordinary genius: but electrical diſcoveries have been made ſo much by accident, that it is more the powers of nature, than of human genius, that excite our wonder with reſpect to them. But if the ſimple electric ſhock would have appeared ſo extraordinary to Sir Iſaac Newton, what would he have ſaid upon ſeeing the effects of a modern electrical battery, and an apparatus for drawing lightning from the clouds! What inexpreſſible pleaſure would it give a modern electrician, were the thing poſſible, to entertain ſuch a man as Sir Iſaac for a few hours with his principal experiments!

To return from this excurſion to the buſineſs of a preface: beſides relating the hiſtory of electrical diſcoveries, in the order in which they were made, I thought it neceſſary, in order to make the work more uſeful, eſpecially to young electricians, to ſubjoin a methodical treatiſe on the ſubject, containing the ſubſtance of the hiſtory in another form, with obſervations and inſtructions of my own. The particular uſes of theſe parts of the work are expreſſed at large in the introductions to them. And, in the laſt place, I have given an account of ſuch original experiments as I have been ſo fortunate as to hit upon myſelf.

I INTITLE the work the *Hiſtory and Preſent State of Electricity*; and whether there be any more additions of the whole work or not, care will be taken to preſerve the propriety of the title, by occaſionally printing ADDITIONS, in the ſame ſize, as new diſcoveries are made; which will always be ſold at a reaſonable price to the purchaſers of the book; or given *gratis*, if the bulk be inconſiderable.

CONSIDERING what reſpectable perſons have already honoured this work with their valuable communications, I hope it will not be deemed arrogance in me, if I here advertiſe, that if any perſon ſhall make diſcoveries in electricity which he would chuſe to ſee recorded in this hiſtory, he will oblige me by a communication of them; and if they be really original, a proper place ſhall certainly be aſſigned to them in the next edition, or paper of additions. And I hope that, if electricians in general would fall into this method, and make either a periodical, or occaſional, but joint communication of their diſcoveries to the public, the greateſt advantage would thence accrue to the ſcience.

The buſineſs of philoſophy is ſo multiplied, that all the books of general philoſophical tranſactions cannot be purchaſed by many perſons, or read by any perſon. It is high time to *ſubdivide* the buſineſs, that every man may have an opportunity of ſeeing every thing that relates to his own favourite purſuit; and all the various branches of philoſophy would find their account in this amicable ſeparation. Thus the numerous branches of a large overgrown family, in the patriarchal ages, found it neceſſary to ſeparate; and the convenience of the whole, and the ſtrength, and increaſe of each branch were promoted by the ſeparation. Let the youngeſt daughter of the ſcience ſet the example to the reſt, and ſhow that ſhe thinks herſelf conſiderable enough to make her appearance in the world without the company of her ſiſters.

But before this general ſeparation, let each collect together every thing that belongs to her, and march off with her whole ſtock. To drop the alluſion: let hiſtories be written of all that has been done in every particular branch of ſcience, and let the whole be ſeen at one view. And when once the entire progreſs, and preſent ſtate of every ſcience ſhall be fully and fairly exhibited, I doubt not but we ſhall ſee a new and capital *æra* commence in the hiſtory of all the ſciences. Such an eaſy, full, and comprehenſive view of what has been done hitherto could not fail to give new life to philoſophical inquiries. It would ſuggeſt an infinity of new experiments, and would undoubtedly greatly accelerate the progreſs of knowledge; which is at preſent retarded, as it were, by its own weight, and the mutal entanglement of its ſeveral parts.

I will juſt throw out a farther hint, of what, I think, might be favourable to the increaſe of philoſophical knowledge. At preſent there are, in different countries in Europe, large incorporate ſocieties, with funds for promoting philoſophical knowledge in general. Let philoſophers now begin to ſubdivide themſelves, and enter into ſmaller combinations. Let the ſeveral companies make ſmall funds, and appoint a director of experiments. Let every member have a right to appoint the trial of experiments in ſome proportion to the ſum he ſubſcribes, and let a periodical account be publiſhed of the reſult of them all, ſucceſsful or unſucceſsful. In this manner, the powers of all the members would be united and increaſed. Nothing would be left untried, which could be compaſſed at a moderate expence, and it being *one perſon's buſineſs* to attend to theſe experiments, they would be made, and reported without loſs of time. Moreover, as all incorporations in theſe ſmaller ſocieties ſhould be avoided, they would be encouraged only in proportion as they were found to be uſeful; and ſucceſs in ſmaller things would excite them to attempt greater.

I by no means diſapprove of large, general, and incorporated ſocieties. They have their peculiar uſes too; but we ſee by experience, that they are apt to grow too large, and their forms are too ſlow for the diſpatch of the *minutiæ* of buſineſs, in the preſent multifarious ſtate of philoſophy. Let recourſe be had to rich incorporated ſocieties, to defray the expence of experiments, to which the funds of ſmaller ſocieties ſhall be unequal. Let their tranſactions contain a ſummary of the more important diſcoveries, collected from the ſmaller periodical publications. Let them, by rewards, and other methods, encourage thoſe who diſtinguiſh themſelves in the inferior ſocieties; and thus give a general attention to the whole buſineſs of philoſophy.

I wish all the incorporated philoſophical ſocieties in Europe would join their funds (and I wiſh they were ſufficient for the purpoſe) to fit out ſhips for the complete diſcovery of the face of the earth, and for many capital experiments which can only be made in ſuch extenſive voyages.

Princes will never do this great buſineſs to any purpoſe. The ſpirit of adventure ſeems to be totally extinct in the preſent race of merchants. This diſcovery is a grand deſideratum in ſcience; and where may this pure and noble enthuſiaſm for ſuch diſcoveries be expected but among philoſophers, men uninfluenced by motives either of policy or gain? Let us think ourſelves happy if princes give no obſtruction to ſuch deſigns. Let them fight for the countries when they are diſcovered, and let merchants ſcramble for the advantage that may be made of them. It will be an acquiſition to philoſophers if the ſeat of war be removed ſo far from the ſeat of ſcience; and freſh room will be given to the exertion of genius in trade, when the old beaten track is deſerted, when the old ſyſtem of traffic is unhinged, and when new and more extenſive plans of commerce take place. I congratulate the preſent race of philoſophers on what is doing by the Engliſh court in this way *; for with whatever view expeditions into the South Seas are made, they cannot but be favourable to philoſophy.

NATURAL PHILOSOPHY is a ſcience which more eſpecially requires the aid of wealth. Many others require nothing but what a man's own reflection may furniſh him with. They who cultivate them find within themſelves every thing they want. But experimental philoſophy is not ſo independent. Nature will not be put out of her way, and ſuffer her materials to be thrown into all that variety of ſituations which philoſophy requires, in order to diſcover her wonderful powers, without trouble and expence. Hence the patronage of the great is eſſential to the flouriſhing ſtate of this ſcience. Others may project great improvements, but they only have the power of carrying them into execution.

BESIDES, they are the higher claſſes of men which are moſt intereſted in the extenſion of all kinds of natural knowledge; as they are moſt able to avail themſelves of any diſcoveries, which lead to the felicity and embelliſhment of human life. Almoſt all the elegancies of life are the produce of thoſe polite arts, which could have had no exiſtence without natural ſcience, and which receive daily improvements from the ſame ſource. From the great and the opulent, therefore, theſe ſciences have a natural claim for pro-

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* Written in the year 1766.

tection; and it is evidently their intereſt not to ſuffer promiſing inquiries to be ſuſpended for want of the means of proſecuting them.

But other motives, beſides this ſelfiſh one, may reaſonably be ſuppoſed to attach perſons in the higher ranks of life to the ſciences; motives more exalted, and flowing from the moſt extenſive benevolence. From Natural Philoſophy have flowed all thoſe great inventions, by means of which mankind in general are able to ſubſiſt with more eaſe, and in greater numbers upon the face of the earth. Hence ariſe the capital advantages of men above brutes, and of civilization above barbarity. And by theſe ſciences alſo it is, that the views of the human mind itſelf are enlarged, and our common nature improved and ennobled. It is for the honour of the ſpecies, therefore, that theſe ſciences ſhould be cultivated with the utmoſt attention.

And of whom may theſe enlarged views, comprehenſive of ſuch great objects, be expected, but of thoſe whom divine providence has raiſed above the reſt of mankind. Being free from moſt of the cares peculiar to individuals, they may embrace the intereſts of the whole ſpecies, feel for the wants of mankind, and be concerned to ſupport the dignity of human nature.

Gladly would I indulge the hope, that we ſhall ſoon ſee theſe motives operating in a more extenſive manner than they have hitherto done; that by the illuſtrious example of a few, a taſte for natural ſcience will be excited in many, in whom it will operate the moſt effectually to the advantage of ſcience and of the world; and that all kinds of philoſophical inquiries will, henceforward, be conducted with more ſpirit, and with more ſucceſs than ever.

Were I to purſue this ſubject, it would carry me far beyond the reaſonable bounds of a preface. I ſhall therefore conclude with mentioning that ſentiment, which ought to be uppermoſt in the mind of every philoſopher, whatever be the immediate object of his purſuit; that ſpeculation is only of uſe as it leads to *practice*, that the immediate uſe of natural ſcience is the power it gives us over nature, by means of the knowledge we acquire of its laws; whereby human life is, in its preſent ſtate, made more comfortable and happy; but that the greateſt, and nobleſt uſe of philoſophical ſpeculation is the diſcipline of the heart, and the opportunity it affords of inculcating benevolent and pious ſentiments upon the mind.

A PHILOSOPHER ought to be ſomething greater, and better than another man. The contemplation of the works of God ſhould give a ſublimity to his virtue, ſhould expand his benevolence, extinguiſh every thing mean, baſe, and ſelfiſh in his nature, give a dignity to all his ſentiments, and teach him to aſpire to the moral perfections of the great author of all things. What great and exalted beings would philoſophers be, would they but let the objects about which they are converſant have their proper moral effect upon their minds! A life ſpent in the contemplation of the productions of divine power, wiſdom, and goodneſs, would be a life of devotion. The more we ſee of the wonderful ſtructure of the world, and of the laws of nature, the more clearly do we comprehend their admirable uſes, to make all the percipient creation happy: a ſentiment, which cannot but fill the heart with unbounded love, gratitude, and joy.

EVEN every thing painful and diſagreeable in the world appears to a philoſopher, upon a more attentive examination, to be excellently provided, as a remedy of ſome greater inconvenience, or a neceſſary means of a much greater happineſs; ſo that, from this elevated point of view, he ſees all temporary evils and inconveniences to vaniſh, in the glorious proſpect of the greater good to which they are ſubſervient. Hence he is able to venerate and rejoice in God, not only in the bright ſunſhine, but alſo in the darkeſt ſhades of nature, whereas vulgar minds are apt to be diſconcerted with the appearance of evil.

NOR is the cultivation of piety uſeful to us only as *men*, it is even uſeful to us as *philoſophers:* and as true philoſophy tends to promote piety, ſo a generous and manly piety is reciprocally, ſubſervient to the purpoſes of philoſophy; and this both in a direct and indirect manner. While we keep in view the great final cauſe of all the parts and the laws of nature, we have ſome clue, by which to trace the efficient cauſe. This is moſt of all obvious in that part of philoſophy which reſpects the animal creation. As the great and excellent Dr. Hartley obſerves. "Since this world is a ſyſtem of "benevolence, and conſequently its author the object of unbounded "love and adoration, benevolence and piety are our only true "guides in our inquiries into it; the only keys which will unlock "the myſteries of nature, and clues which lead through her labyrinths. Of this all branches of natural hiſtory, and natural philoſophy afford abundant inſtances. In all theſe inquiries, let the "inquirer take it for granted previouſly, that every thing is right, "and the beſt that can be *ceteris manentibus*; that is, let him, with "a pious confidence, ſeek for benevolent purpoſes, and he will be "always directed to the right road; and after a due continuance "in it, attain to ſome new and valuable truth: whereas every other "principle and motive of examination, being foreign to the great "plan on which the univerſe is conſtructed, muſt lead into endleſs "mazes, errors, and perplexities *."

WITH reſpect to the indirect uſe of piety, it muſt be obſerved, that the tranquility, and chearfulneſs of mind, which reſults from devotion forms an excellent temper for conducting philoſophical inquiries; tending to make them both more pleaſant, and more ſucceſsful. The ſentiments of religion and piety tend to cure the mind of envy, jealouſy, conceit, and every other mean paſſion, which both diſgrace the lovers of ſcience, and retard the progreſs of it, by laying an undue bias upon the mind, and diverting it from the calm purſuit of truth.

LASTLY, let it be remembered, that a taſte for ſcience, pleaſing, and even honourable as it is, is not one of the higheſt paſſions of our nature, that the pleaſures it furniſhes are even but one degree above thoſe of ſenſe, and therefore that temperance is requiſite in all ſcientifical purſuits. Beſides the duties of every man's proper ſtation in life, which ought ever to be held ſacred and inviolate, the calls of piety, common friendſhip, and many other avocations ought generally to be heard before that of ſtudy. It is, therefore, only a ſmall ſhare of their leiſure, that moſt men can be juſtified in giving to the purſuit of ſcience; though this ſhare is more or leſs, in proportion to a man's ſituation in life, his natural abilities, and the opportunity he has for conducting his inquiries.

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* Hartley's Obſervations on Man, Vol. ii. p. 245.

I SHALL conclude with another paſſage from Dr. Hartley to this purpoſe. "Though the purſuit of truth be an entertainment and "employment ſuitable to our rational natures, and a duty to him who "is the fountain of all knowledge and truth, yet we muſt make frequent intervals and interruptions; elſe the ſtudy of ſcience, without a view to God and our duty, and from a vain deſire of applauſe, will get poſſeſſion of our hearts, engroſs them wholly, "and by taking deeper root than the purſuit of vain amuſements, "become, in the end, a much more dangerous, and obſtinate evil "than that. Nothing can eaſily exceed the vain-glory, ſelf-conceit, arrogance, emulation, and envy, that are found in the eminent profeſſors of the ſciences, Mathematics, Natural Philoſophy, "and even Divinity itſelf. Temperance in theſe ſtudies is, therefore, evidently required, both in order to check the riſe of ſuch ill "paſſions, and to give room for the cultivation of other eſſential "parts of our natures. It is with theſe pleaſures as with the ſenſible ones; our appetites muſt not be made the meaſure of our "indulgence, but we ought to refer all to a higher rule.

"BUT when the purſuit of truth is directed by this higher rule, "and entered upon with a view to the glory of God, and the good "of mankind, there is no employment more worthy of our natures, or more conducive to their purification and perfection [*]."

WARRINGTON,
March, 1767.

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[*] Hartley's Obſervations on Man, Vol. ii. p. 255, &c.

THE

PREFACE

TO THE

SECOND EDITION.

THE method I took to diſtinguiſh the books I had ſeen from thoſe I had not ſeen, in the catalogue of electric authors, ſubjoined to the firſt edition of this work, has been attended with the advantage I promiſed myſelf from it; ſeveral perſons, who were in poſſeſſion of the books I had not ſeen, having communicated them to me; and I have carefully peruſed them, and digeſted their contents into this ſecond edition. Far the greater part of theſe new authors, the reader will perceive by the catalogue, were German, and wrote in High Dutch, a language with which, I believe, the literati of this country are but little acquainted, which might be the reaſon why neither myſelf nor my friends had ever heard of them before. Though the new materials they have ſupplied cannot be ſaid to be of the firſt importance, many of the articles are very curious, and I hope the reader, as well as myſelf, will think that they have well repaid me for my trouble in learning the language.

It is certainly much to be regretted that philoſophers have not one common language; but neither the theory of language in general, nor the nature and analogies of things to be expreſſed by it are yet ſufficiently underſtood, to enable us to contrive a new and philoſophical one, which might be eaſily learnt, and would be completely adequate to all the purpoſes of ſcience; and Latin is a language which perſons of a philoſophical turn of mind have ſeldom leiſure to make themſelves ſo much maſters of, as to write in it with that elegance which the taſte of the age requires. Beſides, books written in Latin are but little read, at leaſt in England; and therefore could have no ſale with us. Theſe circumſtances make it the more neceſſary, that there ſhould be, in every country, perſons poſſeſſed of a competent knowledge of foreign languages, who ſhould be attentive to the progreſs of ſcience abroad, and communicate to their countrymen all uſeful diſcoveries as they are made.

Besides the improvements in the hiſtory, and other parts of this work, the reader will find, in this edition, an addition of three intire ſections of original experiments. All that are of the leaſt conſequence are printed, and ſold ſeparately, for the benefit of thoſe who purchaſed the firſt edition.

Leeds, Jan. 1769.

THE

PREFACE

TO THE

THIRD EDITION.

TO the ſecond edition of this work I made conſiderable additions, by an account of ſuch diſcoveries as had been made in the interval between that and the firſt edition; and theſe *additions* I publiſhed ſeparately for the uſe of thoſe who had purchaſed the firſt edition. But in this edition I have inſerted no account of any thing that was done after the publication of the ſecond, becauſe I reſerve an account of them for a *Continuation of the Hiſtory*, which, if God ſpare my life, I propoſe to write ſome years hence, when I hope there will be a greater ſtock of materials for it.

I HAVE, however, conſiderably improved this edition, by a fuller account of diſcoveries made by ſeveral foreigners, in conſequence of becoming poſſeſſed of the original publications, whereas before I was obliged to content myſelf with quoting them at ſecond hand. The reader will therefore find a much larger account of what was done by the Academicians del Cimento, by Mr. Du Fay, and ſome others. The alterations of the references, or the additions to them, will generally ſhow where I have done this *.

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* In the account of the experiments of Mr. Monnier, thinking proper to change the place of one of the articles, I neglected to cancel the former account, which the reader will pleaſe to overlook, or expunge, from p. 116. The fuller account, from the original, is at p. 124. I mention this chiefly, that if any foreigner ſhould tranſlate from this edition, he may be admoniſhed to omit the former paragraph.

To the account I have given of the reception of Dr. Franklin's ſyſtem in France, I would add what I have ſince been informed of, viz. that Mr. Le Roi, ſecretary to the Royal Academy of Sciences, who has diſtinguiſhed himſelf by his attention to various branches of philoſophy, was the firſt who adopted this theory in that country, and became an open and ſtrenuous advocate for it. He alſo demonſtrated the principles of it by original experiments; an account of which is contained in two valuable Memoirs publiſhed among thoſe of the Royal Academy. In the former of theſe he proves, that there is an invariable diſtinction between the appearance of electric light at the points of metalic bodies as *connected with*, or *preſented to* the prime conductor, or inſulated rubber; ſo that what is called the *pencil*, is uniformly the appearance when the pointed wire is electrified poſitively, and the *ſtar* when it is negative. See Ac. Par. 1753. In another memoir for the year 1755, he ſhews, by uſing a globe of ſulphur, that the reſinous electricity of Mr. Du Fay is the very ſame thing with the negative power of Dr. Franklin. Though the ſame things had been demonſtrated by others, and eſpecially Father Beccaria; theſe philoſophers made the diſcovery independent of each other, and therefore have equal merit.

Without entering into particulars, I ſhall take the liberty to acquaint the reader, that, in the ſixtieth volume of the Philoſophical Tranſactions, there are two papers of mine on electrical ſubjects, one on what I have called the *lateral exploſion*, and the other on the *conducting power of charcoal.*

In the former I ſhew that, in certain circumſtances, an electric ſpark detaches itſelf from the circuit of an exploſion to bodies placed near it, and returns to it again at the ſame inſtant. Through the air I have made this ſpark three-fourths of an inch in length, and in vacuo more than twelve inches.

In the other paper I ſhow, among other properties of charcoal, that its conducting power depends entirely on the degree of *heat* with which it is made. Some pieces do not conduct electricity at all, others as perfectly as ſilver or gold, and pieces of the former quality are always convertible into thoſe of the latter, by the application of more heat.

Also, in my *obſervations on different kinds of air*, the reader will find, that I have demonſtrated that the electric matter is, or contains phlogiſton; by ſhewing that it affects all kinds of air as phlogiſton does; particularly diminiſhing common air one fourth, and making it noxious, ſo as to make no effervescence with nitrous air.

It may not be amiſs to inform the reader, that the firſt tranſlation of this work was into French, by a perſon who ſeems to have done it with no other view than to have an opportunity of expreſſing his diſlike of Dr. Franklin's ſyſtem, and of myſelf as the abettor of it, and of defending that of Mr. Nollet. In the *notes*, which all other tranſlators cenſure as in the higheſt degree illiberal, he repreſents me as being, beyond all bounds, partial to my own countrymen, and particularly unjuſt to the French; a charge from which I thought myſelf intirely exempt. I confeſs, however, that, inadvertently, I did give ſome handle to this cenſure in one paſſage, which I have therefore corrected in this edition. I have the pleaſure to be informed that a new tranſlation of this work, from this laſt edition, is undertaken by the excellent tranſlator of Dr. Franklin's Philoſophical writings, lately publiſhed. This gentleman will, I doubt not, do ample juſtice both to myſelf and to the ſubject.

Calne, March, 1775.

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THE HISTORY AND PRESENT STATE OF ELECTRICITY.

PART I.

THE HISTORY OF ELECTRICITY.

PERIOD I.

EXPERIMENTS AND DISCOVERIES IN ELECTRICITY PRIOR TO THOSE OF MR. HAUKSBEE.

THE hiſtory of philoſophy contains nothing earlier than the obſervation, that yellow amber, when rubbed, has the power of attracting light bodies. Thales of Miletus, the father of the Ionic philoſophy, who flouriſhed about ſix hundred years before Chriſt, was ſo much ſtruck with this property of amber, that he imagined it was animated. But the firſt writer, who expreſsly mentions this ſubſtance, is Theophraſtus, who flouriſhed about the year 300 before Chriſt. He ſays, in his book concerning precious ſtones, ſect. 53, that amber (which he ſuppoſes to be a native foſſil) has the ſame property of attracting light bodies with the lyncurium; which, he obſerves, attracts not only ſtraws, and ſmall pieces of ſticks, but even thin pieces of copper and iron. What he ſays farther of the lyncurium will be related under the article of the *tourmalin*, which Dr. Watſon has, in a manner, proved to be the ſame ſubſtance.

FROM ηλεκτρον, the Greek name for amber, is derived the term ELECTRICITY, which is now extended to ſignify not only the power of attracting light bodies inherent in amber, but other powers connected with it, in whatever bodies they are ſuppoſed to reſide, or to whatever bodies they may be communicated.

THE attractive nature of amber is occaſionally mentioned by Pliny, and other later naturaliſts; particularly by Gaſſendus, Kenelm Digby, and Sir Thomas Brown; but excepting the electricity of the ſubſtance called *jet* *, the diſcovery of which was very late (though I have not been able to find its author) no advances were made in electricity till the ſubject was undertaken by William Gilbert, a native of Colcheſter, and a phyſician at London; who, in his excellent Latin treatiſe *de magnete*, publiſhed in the year 1600, relates a great variety of electrical experiments. Conſidering the time in which this author wrote, and how little was known of the ſubject before him, his diſcoveries may be juſtly deemed conſiderable, though they appear trifling when compared with thoſe which have been made ſince his time.

To him we owe a great augmentation of the liſt of electric bodies, as alſo of the bodies on which electrics can act; and he has carefully noted ſeveral capital circumſtances relating to the manner of their action, though his theory of electricity was very imperfect, as might be expected.

AMBER and jet were, as I obſerved before, the only ſubſtances which, before the time of Gilbert, were known to have the property of attracting light bodies when rubbed; but he found the ſame property in the *diamond*, *ſapphire*, *carbuncle*, *iris*, *amethyſt*, *opal*, *vincentina*, *Briſtol ſtone*, *beryl*, and *cryſtal*. He alſo obſerves that *glaſs*, eſpecially that which is clear and tranſparent, has the ſame property; likewiſe all *factitious gems*, made of glaſs or cryſtal; *glaſs of antimony*, moſt *ſparry ſubſtances*, and *belemnites*. Laſtly, he concludes his catalogue of electric ſubſtances with *ſulphur*, *maſtic*, *ſealing wax* made of gum lac tinged with various colours, *hard roſin*, *ſal gem*, *talc*, and *roche alum*. Roſin, he ſaid, poſſeſſed this property but in a ſmall degree, and the three laſt mentioned ſubſtances, only when the air was clear and free from moiſture.

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* Mr. Boſe is ſaid to have ſhown, that the *agate* was very early known to have electric powers. Dantzick Memoirs, Vol. I. p. 179.

ALL theſe ſubſtances, he obſerves, attracted not only ſtraws, but all metals, all kinds of wood, ſtones, earth, water, oil; in ſhort, whatever is ſolid, and the object of our ſenſes. But he imagined that air, flame, bodies ignited, and all matter which was extremely rare was not ſubject to this attraction. Groſs ſmoke, he found, was attracted very ſenſibly, but that which was attenuated very little.

FRICTION, he ſays, is, in general, neceſſary to excite the virtue of theſe ſubſtances; though he had one large and ſmooth piece of amber which would act without friction. But with reſpect to this he probably deceived himſelf. The moſt effectual friction, he obſerved to be that which was light and quick; and he found that electrical appearances were ſtrongeſt when the air was dry, and the wind north or eaſt, at which time electric ſubſtances would act ten minutes after excitation. But he ſays, that a moiſt air, or a ſoutherly wind almoſt annihilates the electric virtue. The ſame effect he alſo obſerved from the interpoſition of moiſture of any kind, as from the breath, and many other ſubſtances, but not always from the interpoſition of ſarſnet. He ſays that light and pure oil, ſprinkled upon electrics, after excitation, did not obſtruct their virtue; but that brandy, or ſpirit of wine, did. He alſo ſays, that cryſtal, talc, glaſs, and all other electrics loſt their virtue after being burnt or roaſted. But this was, in ſome meaſure, a miſtake. The heat of the ſun, collected by a burning glaſs, he ſays, is ſo far from exciting amber, and other electrics, that it impairs the virtue of them all; though, when electrics have been excited, they will retain their virtue longer in the ſun-ſhine than in the ſhade.

MOST of the experiments of this author were made with long thin pieces of metal, and other ſubſtances, ſuſpended freely on their centers, to the extremities of which he preſented the electrics he had excited. His experiments on water were made by preſenting a round drop of it upon a dry ſubſtance to the excited electric; and it is remarkable, that he obſerved the ſame conical figure of the electrified drops which Mr. Grey afterwards diſcovered, and which will be related more at large in its proper place. Gilbert concluded, that air was not affected by electrical attraction, becauſe the flame of a candle was not: for the flame, he ſays, would be diſturbed if the air had the leaſt motion given to it.

GILBERT imagined that electrical attraction was performed in the ſame manner as the attraction of coheſion. Two drops of water, he obſerved, ruſh together when they are brought into contact; and electrics, he ſays, are virtually brought into contact with the bodies they act upon, by means of their effluvia, excited by friction.

AMONG other differences between electric and magnetic attraction, ſome of which are very juſt, and others whimſical enough, he ſays, that magnetic bodies ruſh together mutually; whereas in electrical attraction it is only the electric that exerts any power. He obſerves alſo particularly, that in magnetiſm there is both attraction and repulſion, but in electricity only the former, and never the latter[*].

Such were the diſcoveries of our countryman Gilbert, who may juſtly be called the father of modern electricity, though it be true that he left his child in its very infancy.

Lord Bacon, in his Phyſiological Remains, gives a catalogue of bodies attractive and not attractive; but it differs in nothing worth mentioning from that of Gilbert, and he does not ſeem to have made any obſervations of his own relating to the ſubject.

About 30 years after Gilbert, Nicolaus Cabæus, a Jeſuit at Ferrara, repeated his experiments; and found that *white wax*, almoſt all the *gums*, and *crude gypſum* were to be ranked among electric bodies[†].

These remarkable phenomena relating to amber, and other electric ſubſtances, did not eſcape the attention of the inquiſitive and ſagacious Mr. Boyle, who flouriſhed about the year 1670. He made ſome addition to the catalogue of electric ſubſtances, and attended to ſome circumſtances relating to electrical attraction, which had eſcaped the obſervation of philoſophers who lived before him.

He found that the hard cake which remains after evaporating good turpentine was electrical, as alſo the hard maſs which remains after diſtilling petroleum and ſpirit of nitre, glaſs of lead, the caput mortuum of amber, and the cornelian; but he could not find that property in the emerald, and he thought that glaſs poſſeſſed it but in a very low degree.

He found, that the electricity of all bodies capable of having it excited in them was increaſed by wiping, and warming them, previous to their being rubbed. By this means he made an electric body, no bigger than a pea, move a ſteel needle, which was freely poiſed, three minutes after he had left off rubbing it. He alſo found, that it was uſeful to have the ſurfaces of electric bodies made very ſmooth, except in the caſe of one diamond, on which he tried ſome experiments; which, though it was rough, was, he ſays, poſſeſſed of a ſtronger electric virtue than any poliſhed one he had ever met with.

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[*] Gilbert de magnete, Lib. 2. Cap. 2.

[†] Dantzick Memoirs, Vol. 1. p. 180.

HE obſerved that excited electrics would attract all kinds of bodies promiſcuouſly, whether electric or not; that excited amber, for inſtance, would attract both powder of amber, and ſmall pieces of it; differing, as he takes notice, from the property of the load-ſtone, which acts only on one kind of matter. He found, that his electrics would attract ſmoke very eaſily, and takes ſome pains to account for their not ſenſibly attracting flame, which Gilbert excepted from the bodies attracted by electricity.

THESE attractions, he found, did not depend upon the air: for he obſerved that they took place in vacuo. He ſuſpended a piece of excited amber over a light body in a glaſs receiver, and ſaw, that when a vacuum was made, and the amber let down near the light body, it was attracted, as if it had been in the open air *. But S. Beccaria aſſerts, that there is no electrical attraction in a perfect vacuum.

MR. BOYLE made an experiment to try whether an excited electric was acted upon by other bodies, as ſtrongly as it acted upon them, and it ſucceeded: for, ſuſpending his excited electric, he ſaw that it was ſenſibly moved by the approach of any other body. We ſhould now be ſurpriſed that any perſon ſhould not have concluded *a priori*, that if an electric body attracted other bodies, it muſt, in return, be attracted by them, action and reaction being univerſally equal to one another. But it muſt be conſidered, that this axiom was not ſo well underſtood in Mr. Boyle's time, nor till it was afterwards explained in its full latitude by Sir Iſaac Newton *.

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* Hiſtoire de l'ectricité.

MR. BOYLE got a glimpſe, as we may ſay, of the electric light: for he found that a curious diamond, which Mr. Clayton brought from Italy, gave light in the dark, when it was rubbed againſt any kind of ſtuff; and he found that, by the ſame treatment, it became electrical. He alſo obſerved the ſame property in ſeveral other diamonds †.

THESE experiments of Mr. Boyle's, we ſee, relate only to a few circumſtances attending the ſimple property of electrical attraction. The neareſt approach that he made to the diſcovery of electrical repulſion was his obſerving, that light bodies, as feathers, &c. would cling to his fingers, and other ſubſtances, after they had been attracted by his electrics. He had ſeen but little of the electric light, and little imagined what aſtoniſhing effects would be afterwards produced by the ſame wonderful power, and how large a field he was opening for philoſophical ſpeculation in future times.

MR. BOYLE's theory of electrical attraction was, that the electric emitted a glutinous effluvium, which laid hold of ſmall bodies in its way, and, in its return to the body which emitted it, carried them back with it. One James Hartman, whoſe account of amber is publiſhed in the Philoſophical Tranſactions ‡, pretends to prove by experiment, that electrical attraction was really owing to the emiſſion of glutinous particles. He took two electric ſubſtances, viz. pieces of colophonia, and from one of them made a diſtillation of a black balſam, and thereby deprived it of its attractive power. He ſays, that the electric, which was not diſtilled,

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* Boyle's Mechanical production of electricity.

† Secondat's hiſtory of electricity, p. 141.

‡ Abridgement, Vol. 2. p. 473.

diſtilled, retained its fatty ſubſtance, whereas the other was, by diſtillation, reduced to a mere *caput mortuum*, and retained no degree of its bituminous fat. In conſequence of this hypotheſis, he gives it as his opinion, that amber attracts light bodies more powerfully than other ſubſtances, becauſe it emits oily and tenacious effluvia more copiouſly than they do.

Contemporary with Mr. Boyle was Otto Guericke, Burgomaſter of Magdebourg, and the celebrated inventor of the air-pump, who is likewiſe intitled to a diſtinguiſhed place among the firſt improvers of electricity.

This philoſopher made his experiments with a globe of ſulphur, made by melting that ſubſtance in a hollow globe of glaſs, and afterwards breaking the glaſs from off it. He little imagined that the glaſs globe itſelf, with or without the ſulphur, would have anſwered his purpoſe as well. This globe of ſulphur he mounted upon an axis, and whirled it in a wooden frame, rubbing it at the ſame time with his hand; and by this means he performed all the electrical experiments which were known before his time.

His was the diſcovery, that a body once attracted by an excited electric was repelled by it, and not attracted again till it had been touched by ſome other body. In this manner he kept a feather a long time ſuſpended in the air above his ſulphur globe; but he obſerved, that if he drove it near a linen thread, or the flame of a candle, it inſtantly retreated to the globe, without having been in contact with any ſenſible body.

Neither the ſound, nor the light produced by the excitation of his globe, eſcaped the notice of this accurate philoſopher, though he ſeems not to have obſerved them in a very great degree: for he was obliged to hold his ear near the globe to perceive the hiſſing ſound of the electric fire; and he compares the light which it gave in the ſame circumſtances to that which is ſeen when ſugar is pounded in the dark.

BUT the moſt remarkable experiments of this philoſopher were two, which depend upon a property of the electric fluid that has not been illuſtrated till within theſe late years; viz. that bodies immerged in electric atmoſpheres are themſelves electrified, and with an electricity oppoſite to that of the atmoſphere. Threads ſuſpended within a ſmall diſtance of his excited globe, he obſerved to be often repelled by his finger brought near them, and that a feather repelled by the globe always turned the ſame face towards it, like the moon with reſpect to the earth. This laſt experiment ſeems to have been wholly overlooked by later electricians, though it is a very curious one, and may be made with ſo much eaſe *.

TO the members of the Academy *Del Cimento*, whoſe labours contributed very conſiderably to the advancement of various branches of natural knowledge, we are indebted for ſeveral obſervations on the ſubject of electricity.

THEY rank the electric bodies which they examined in the following order, according to the ſtrength of their attractive power, *yellow amber*, *ſealing wax*, the *roſe diamond*, and of the ſame ſtrength with this the *white ſapphire*, *emerald*, *white topaz*, *ſpinelle*, and *ruby balleis*. After theſe they ranked all *tranſparent gems*, and next to the precious ſtones they placed *glaſs*, *cryſtal*, with *white and black amber*, the power of all which they ſay is very weak.

YELLOW amber appearing to them to have the greateſt power of all electric ſubſtances, they made all their experiments with it. Among other things, they found that it attracted *ſmoke*, but not *flame*; and upon this occaſion they obſerved the curious phenomenon of a *viſible electric atmoſphere*, which was afterwards re-diſcovered, and exhibited to more advantage by Dr. Franklin.

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* Experimenta Magdeburgica, Lib. 4. Cap. 15.

For they ſay, that that part of the ſmoke which is attracted by the amber remains, and unites itſelf to it, like a ſmall cloud, and as the amber cools, it riſes in ſmoke again, and vaniſhes. At the ſame time they alſo obſerved a pretty curious effect of electrical repulſion; for they ſay that part of the ſmoke was thrown off from the amber, as from a looking-glaſs.

Flame, they obſerved, was ſo far from being attracted by the amber, that, upon being preſented to it, it preſently deprived it of all its attractive power; for if, after it had taken up any thing, it was held to the flame, it would immediately let it go again.

They found that all *fluid ſubſtances* were ſenſible to the attractive power of amber, and among the reſt even *mercury*; and that when the excited amber was preſented to a large ſuperficies of any liquor, it roſe towards it in a ſmall pointed eminence; an effect which, they ſay, is beſt obſerved in *oil* or *balſam*. This obſervation, we ſhall find, was afterwards made again, and more particularly attended to by Mr. Grey *.

These gentlemen took a great deal of pains to try whether amber would attract in *vacuo*, but to no purpoſe, not being able to exclude the air, ſo as to rub the amber in vacuo, and apply it to the light body they had provided, with any effect; but neither could they make the amber act in the ſame confined ſituation even when the air was not at all excluded †.

They alſo found, with Mr. Boyle, that a piece of excited amber, ſuſpended by a thread, was attracted by other bodies preſented to it, juſt like a magnetical needle ‡.

Lastly, theſe gentlemen found that when the excited amber was dipped in ſome liquors, it immediately loſt its power, but not after being dipped in others. In this laſt claſs they enumerate ſeveral kinds of *oil*, *tallow*, *fat*, and *butter*, and it is now found that the conducting power of theſe ſubſtances is ſo ſmall, that they are more properly claſſed among the non-conductors *.

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* Eſſays on Natural Experiments, tranſlated by Walker, p. 128, &c.

† Ib. p. 43.

‡ Ib. p. 129.

I SHALL in the next place obſerve, that Mr. Boyle, Otto Guericke, and theſe gentlemen, made their experiments about the ſame time, and ſeemed to have derived no advantage whatever from each other's labours.

A MUCH finer appearance of electric light than that which Otto Guericke's ſulphur globe exhibited was obſerved by Dr. Wall. The account of it is publiſhed in the Philoſophical Tranſactions †.

MAKING experiments upon artificial phoſphorus, which he took to be an animal oil coagulated with a mineral acid, he was led to conjecture that amber, which he ſuppoſed to be a mineral oil coagulated with a mineral volatile acid, might be a natural phoſphorus; and with this view he began to make experiments upon it, the reſult of which, being very curious and ſurprizing, it will be moſt agreeable to my readers to ſee in the very words of the obſerver himſelf.

"I FOUND," ſays he, "by gently rubbing a well poliſhed "piece of amber with my hand, in the dark, that it produced "a light: whereupon I got a pretty large piece of amber, "which I cauſed to be made long and taper, and drawing it gent- "ly through my hand, being very dry, it afforded a conſiderable "light.

"I THEN uſed many kinds of ſoft animal ſubſtances, and "found that none did ſo well as wool. And now new pheno- "mena offered themſelves: for, upon drawing the piece of "amber ſwiftly through the woollen cloth, and ſqueezing it "pretty hard with my hand, a prodigious number of little "cracklings were heard, and every one of theſe produced a little

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* Eſſays on Natural Experiments, tranſlated by Walker, p. 131.

† Phil. Tranſ. abridged, Vol. 4. p. 275.

" flaſh of light; but when the amber was drawn gently and " ſlightly through the cloth, it produced only a light but no " crackling; but by holding one's fingers at a little diſtance from " the amber, a large crackling is produced, with a great flaſh " of light ſucceeding it. And, what to me is very ſurpriſing, " upon its eruption, it ſtrikes the finger very ſenſibly, whereſo- " ever applied, with a puſh or a puff, like wind. The crackling " is full as loud as charcoal on fire, and five or ſix cracklings, " or more, according to the quickneſs of placing the finger, have " been produced from one ſingle friction, light always ſucceeding " each of them.

" Now I make no queſtion, but upon uſing a longer and " larger piece of amber, both the cracklings and light would be " much greater, becauſe I never yet found any crackling from " the head of my cane, though it is a pretty large one. This " light and crackling ſeems, in ſome degree, to repreſent thunder " and lightning."

AFTER reciting this experiment, he gives it as his opinion, that all, or moſt bodies, which have electricity, give light, and that it is the light which is the cauſe of their being electrical. He found that light could alſo be produced by rubbing jet, red ſealing wax, made with gum lac and cinnabar, and the diamond. He even imagined he could diſtinguiſh true from falſe diamonds by this teſt.

NOTWITHSTANDING Dr. Wall made this beautiful diſcovery, as he imagined (for he ſeems not to have ſeen what Otto Guericke had written) of light proceeding from amber and other electric bodies, we ſee that he laboured under a great deal of confuſion and miſapprehenſion with reſpect to it. He ſays, that one thing appeared ſtrange to him in the courſe of theſe experiments, which was, that though, upon friction with wool in the day time, the cracklings ſeemed to be full as many and as large, yet that, by all the trials which he made, very little light appeared, though in the darkeſt room. He ſays that the beſt time for making theſe experiments is when the ſun is 18° below the horizon, and that when the ſun was ſo low, though the moon ſhone ever ſo bright, the light was the ſame as in the darkeſt room, which made him chuſe to call it a noctiluca.

It is remarkable that Dr. Wall compares the light and the crackling of his amber to thunder and lightning: ſo early was a ſimilarity between the effects of electricity and lightning obſerved. But little was it imagined that the reſemblance between them extended any farther than to appearances and effects. That the cauſe was the ſame in both, was a diſcovery reſerved for Dr. Franklin, in a much later period.

The great Sir Iſaac Newton, though he by no means makes a principal figure in the hiſtory of electricity, yet made ſome electrical obſervations which engaged the attention of his philoſophical friends; and which, independent of their being made by him, do well deſerve to be tranſmitted to poſterity. They ſeem to ſhew, that he was the firſt who obſerved, that excited glaſs attracted light bodies on the ſide oppoſite to that on which it was rubbed.

Having laid upon the table a round piece of glaſs, about two inches broad, in a braſs ring, ſo that the glaſs might be one eighth of an inch from the table, and there rubbing the glaſs briſkly, little fragments of paper laid on the table, under the glaſs, began to be attracted, and move nimbly to and fro. After he had done rubbing the glaſs, the papers would continue a conſiderable time in various motions; ſometimes leaping up to the glaſs, and reſting there a while, then leaping down and reſting there, and then leaping up and down again; and this ſometimes in lines ſeemingly perpendicular to the table, ſometimes in oblique ones; ſometimes alſo leaping up in one arch, and leaping down in another, divers times together, without ſenſibly reſting between; ſometimes ſkipping in a bow from one part of the glaſs to another, without touching the table, and ſometimes hanging by a corner, and turning often very nimbly, as if they had been carried about in the midſt of a whirlwind, and being otherwiſe variouſly moved, every paper with a different motion. Upon his ſliding his finger on the upper ſide of the glaſs, though neither the glaſs, nor the incloſed air below, were moved, yet he obſerved, that the papers, as they hung under the glaſs, would receive ſome new motion, inclining this way or that, according as he moved his finger.

Some of the motions, as that of hanging by a corner and twirling about, and that of leaping from one part of the glaſs to another without touching the table happened but ſeldom; but, he ſays, it made him take the more notice of them *.

An account of this experiment Sir Iſaac ſent to the members of the R. Society in the year 1675, deſiring it might be tried by them; and after ſome ineffectual attempts, and receiving further inſtructions how to make it, they at length ſucceeded, and the thanks of the ſociety were formally returned to him †.

Upon repeating the experiment with ſome variety of circumſtances, Sir Iſaac obſerves, that rubbing variouſly, or with various things, altered the caſe. At one time he rubbed a glaſs four inches broad, and one fourth thick, with a napkin, twice as much as he uſed to do with his gown, and nothing would ſtir, and yet preſently after, rubbing with ſomething elſe, the motion ſoon began. After the glaſs had been much rubbed, he thought the motions were not ſo laſting, and the day following he found the motions fainter, and more difficult to be excited than at firſt ‡.

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* Birch's Hiſt. of the R. Society, Vol. 3. p. 260, &c. † Ib. 271. ‡ Ib. p. 270.

SIR ISAAC alſo mentions electricity in two queries annexed to his treatiſe on Optics, from which we learn, that he imagined electric bodies when excited emitted an elaſtic fluid, which freely penetrated glaſs, and that the emiſſion was performed by the vibratory motions of the parts of the excited bodies *.

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* Newton's Optics, octavo, p. 314, and 327.

PERIOD II.

THE EXPERIMENTS AND DISCOVERIES OF MR. HAUKSBEE.

AFTER Gilbert, Mr. Boyle, and Otto Guericke, Mr. Haukſbee, who wrote in the year 1709, diſtinguiſhed himſelf by experiments and diſcoveries in electricity. He firſt obſerved the great electric power of glaſs, the light proceeding from it, and the noiſe occaſioned by it, together with a variety of phenomena relating to electrical attraction and repulſion. He was indefatigable in making experiments, and there are few perſons to whom we are more indebted for a real advancement of this branch of knowledge. This will appear from the following ſuccinct account of his experiments, related not exactly in the order in which he has publiſhed them, but according to their connection. This method I have choſen, as beſt adapted to give the moſt diſtinct view of the whole.

I SHALL firſt relate the experiments he made concerning *electrical attraction and repulſion*, in many of which we ſhall ſee reaſon to admire his ingenious contrivances, and ſhall ſee that little was added to his obſervations, till the capital diſcovery of a *plus* and *minus* electricity by Dr. Watſon and Dr. Franklin, and the farther illuſtration of that doctrine by Mr. Canton.

THE moſt curious of his experiments concerning electrical attraction and repulſion are thoſe which ſhew the direction in which thoſe powers are exerted.

HAVING tied threads round a wire hoop, and brought it near to an excited globe or cylinder, he obſerved, that the threads kept a conſtant direction towards the center of the globe, or towards ſome point in the axis of the cylinder, in every poſition of the hoop; that this effect would continue for about four minutes after the whirling of the globe ceaſed, and that the effect was the ſame whether the wire was held above or under the glaſs; or whether the glaſs was placed with its axis parallel, or perpendicular to the horizon.

HE obſerved, that the threads pointing towards the center of the globe were attracted and repelled by a finger preſented to them; that if the finger, or any other body, was brought very near the threads, they would be attracted; but that if it were brought to the diſtance of about an inch, they would be repelled, the reaſon of which difference he did not ſeem to underſtand *.

HE tied threads to the axis of a globe and cylinder, and found that they diverged every way in ſtraight lines from the place where they were tied, when the globe was whirled and rubbed. In both caſes, he ſays, the threads would be repelled by the finger held on the oppoſite ſide of the glaſs, even without touching the glaſs; though they would ſometimes ſuddenly jump towards it †. He obſerved farther, that by blowing with his mouth towards the glaſs, at three or four feet diſtance, the threads would have a very conſiderable motion given to them.

HE found that threads, hanging freely in an unexcited globe, at reſt, would be moved by the approach of any excited electric at a conſiderable diſtance, except in moiſt weather; which failure he accounts for, by ſuppoſing the moiſture on the ſurface

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* Phyſico-Mechanical Experiments, p. 75.

† Ib. p. 78.

of the glaſs prevented the free paſſage of the electric effluvia through it [*].

The varieties he obſerved on the appearances and properties of the *electric light*, are even more curious and ſurpriſing than his diſcoveries concerning electric attraction and repulſion; and it is ſomething remarkable that Mr. Haukſbee's tranſition to electric light was like that of Dr. Wall, viz. from the light of phoſphorus.

Mr. Hauksbee firſt produced a conſiderable quantity of light by ſhaking quick-ſilver in a glaſs veſſel, out of which the air was exhauſted. Sometimes what he calls ſtrange flaſhes of pale light were ſeen darting in a variety of directions, when the mercury was put in motion within an exhauſted receiver [†]. But the diſcovery was probably accidental, and he did not ſeem, at that time, to know the reaſon of the appearance. He called this light the *mercurial phoſphorus*, and did not conſider the glaſs as any way concerned in producing it.

He alſo found, that this appearance of electric light (which he ſtill calls the mercurial phoſphorus) did not require a very perfect vacuum, nor even a near approach to it [‡]. Nay he ſometimes produced that appearance of light by ſhaking mercury in a veſſel, in which the air was of the ſame denſity with the external atmoſphere; but ſtill he had no idea of the glaſs contributing to the phenomenon [||].

He obſerved a ſtrong light in vacuo, and a ſmall one in the open air, from rubbing amber upon woollen, but ſeems to have conſidered it as any hard body rubbing againſt a ſoft one [§]. He alſo obſerved a vivid purple, and afterwards a pale light produced by rubbing glaſs upon woollen in vacuo *. He ſays that every freſh glaſs firſt gave a purple, and then a pale light, and that woollen tinctured with ſalt or ſpirits produced a new, ſtrong, and fulgurating light †.

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[*] Phyſico-Mechanical Experiments, p. 160. [†] Ib. 12. [‡] Ib. 14.
[||] Ib. p. 18. [§] Ib. 26.

IN the following experiments we find his ideas of electric light much more diſtinct, and the appearances the ſame that are uſually exhibited by our preſent electrical machines, the ſtructure of which, we ſhall find to be nearly the ſame with thoſe which he uſed.

HE provided himſelf with a machine in which he could whirl a glaſs globe; and he obſerved, when the air was extracted out of it, that, upon applying his hand to the globe, a ſtrong light appeared on the inſide, and, upon letting in the air, he obſerved the light on the outſide alſo; but with ſome very conſiderable differences in its appearances, ſtriking upon his fingers, and other bodies held near the globe. He alſo obſerved, upon this occaſion, that one fourth of an atmoſphere in the globe did very little diminiſh the light within. It is pleaſing to obſerve, that the ſimilar appearance in this experiment, and that with mercury in vacuo before-mentioned, made him ſuſpect, though only ſuſpect, that the light produced in the former caſe proceeded not from the mercury, but from the glaſs.

THE next experiment is of a very delicate and curious nature. It is not to be wondered at, that Mr. Haukſbee did not underſtand the circumſtances which contributed to it, as the explication of it depends upon principles which were not diſcovered till a much later period by Mr. Canton.

HOLDING an exhauſted globe within the effluvia of an excited one, he obſerved a light in the exhauſted globe, which preſently died away, if it was kept at reſt; but which was revived, and continued very ſtrong, if the exhauſted globe was kept in motion.

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* Phyſico-Mechanical Experiments, p 32.

† Ib. 34.

tion. Preſenting an exhauſted tube to the effluvia of an excited globe, it produced what he calls an interrupted flaſhing light. He imagined that the exhauſted globe was excited by the attraction of the effluvia from the other globe, ſo little did he underſtand the true cauſe of this curious experiment *. When he ſays that light is producible by the effluvia of one glaſs falling upon another, he adds; that electric (by which he means attractive) matter, is not to be brought forth by any ſuch feeble ſtrokes. He had before obſerved that, upon rubbing an exhauſted tube, it diſcovered no attractive power, nor gave any light outwards, but only inwards.

He found that when the friction was performed in vacuo, no electricity (that is attraction) could be produced †; but that though the *attractive quality* required the preſence both of the external and internal air, in order to its ſhewing itſelf, yet the *light* requires the preſence of only one of them in order to its appearance; for that either a glaſs globe full of air rubbed *in vacuo*, or with its air exhauſted and rubbed *in pleno*, would produce a very conſiderable light ‡.

He ſays alſo, that thoſe lights are leſs ſenſibly affected by the return of air, which are produced by the attrition of exhauſted glaſs in pleno, than thoſe which are produced by the attrition of glaſs full of air in vacuo; for that, in the former caſe, no great alteration was found in the light or colour, until a certain quantity of air was let into the inſide of the exhauſted glaſs; but that, in the latter caſe, both light and colour were ſenſibly changed, upon every admiſſion of air to the outſide of the full glaſs §.

The greateſt electric light Mr. Haukſbee produced, was when he encloſed one exhauſted cylinder within another not exhauſted, and excited the outermoſt of them, putting them both in motion. Whether their motions conſpired or not, he obſerved, made no difference. When the outer cylinder only was in motion, he ſays, the light was very conſiderable, and ſpread itſelf over the ſurface of the inner glaſs. What ſurpriſed him moſt was, that after both glaſſes had been in motion ſome time, during which his hand had been applied to the ſurface of the outer glaſs, the motion of both ceaſing, and no light at all appearing; if he did but bring his hand again near the ſurface of the outer glaſs, there would be flaſhes of light, like lightning, produced in the inner glaſs; as if, he ſays, the effluvia from the outer glaſs had been puſhed with more force upon it by means of the approaching hand*. This experiment was ſimilar to thoſe which he made with the excited and exhauſted globe, and with the exhauſted tube; and his reaſoning upon it ſhews, that he was ſtill far from being fully apprized of all the circumſtances attending this fact.

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* Phyſico-Mechanical Experiments, p. 82. † Ib. 242. ‡ Ib. 248. § Ib. p. 248.

THE next experiments which I ſhall relate of Mr. Haukſbee's, are thoſe which ſhew the great copiouſneſs, and extreme ſubtilty of electric light. They are really amazing, and have not yet been purſued in the manner they deſerve.

HE lined more than half of the inſide of a glaſs globe with ſealing wax, and having exhauſted the globe, he put it in motion; when, applying his hand to excite it, he ſaw the ſhape and figure of all the parts of his hand diſtinctly and perfectly, on the concave ſuperficies of the wax within. It was as if there had only been pure glaſs, and no wax interpoſed between his eye and his hand. The lining of wax, where it was ſpread the thinneſt, would but juſt allow the ſight of a candle through it in the dark; but in ſome places the wax was, at leaſt, one eighth of an inch thick; yet even in thoſe places, the light and the figure of his hand were as diſtinguiſhable through it, as any where elſe. Nay though, in ſome places, the ſealing wax did not adhere ſo cloſely to the glaſs as in others, yet the light on theſe appeared juſt as on the reſt [*].

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* Phyſico-Mechanical Experiments, p. 87.

THESE experiments ſucceeded equally with pitch inſtead of ſealing wax. And he obſerved, that when the air was let into the glaſs, every part of it, the lined part and the unlined, ſeemed to attract with equal vigour [†]. Melted flowers of ſulphur had no ſuch effect, but common ſulphur anſwered as well as ſealing wax, or pitch. In both theſe laſt experiments the ſulphur was found to have been ſeparated from the glaſs [‡].

USING a large quantity of common ſulphur in the ſame manner, the light in the inſide was four times as great, but the figure of his fingers was not ſo diſtinguiſhable as in the former caſes. He likewiſe obſerved, that on the part near the axis, where the ſubſtance of the ſulphur was the greateſt, no light was produced; which he attributed, chiefly, to the ſlowneſs of the motion in that place [§].

UPON the admiſſion of a ſmall quantity of air into the globe, thus partially lined with ſealing wax, the light wholly diſappeared on the part covered with the wax, but not on the other.

HE alſo obſerved, that when all the air was let in, and the hoop of threads before-mentioned held over the glaſs, the threads were attracted at greater diſtances by the part which was coated with the wax than by the other: when all the air was exhauſted, he ſays, the wax would attract bodies placed near the out-ſide of the glaſs; that even in this caſe, the threads preſerved their central direction, though not ſo vigorouſly as when the air was let in; but that they would not be attracted at all, when there was no wax on the inſide of the exhauſted globe.

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[*] Phyſico-Mechanical Experiments, p. 168.

[†] Ib. 269.

[‡] Ib. 274.

[§] Ib. 275.

MR. HAUKSBEE was not unattentive to the *ſound* made by the emiſſion of the electric effluvia, or to the manner in which it affected the ſenſe of *feeling*. He obſerved, that when an excited tube of glaſs attracted various bodies, and threw light upon them, as they were held near it, a noiſe, which he calls a *ſnapping*, was likewiſe heard. He alſo ſays, that the rubbed tube, held near the face, gave a feeling, as if fine hairs had been drawn over it; and when he repeated the experiment of whirling and rubbing the glaſs globe, he obſerved the light to proceed from it with ſome noiſe, and to make a kind of preſſure upon the finger, when it was held within half an inch of it *.

NOR was MR. Haukeſbee's attention confined to the electric power of *glaſs*. He made experiments with a globe of *ſealing wax*, in the center of which was a globe of wood; from which he concluded, that the electricity of ſealing wax was, in general the ſame with that of glaſs, but different from it in degree. He could not make any light adhere to his finger when preſented to the excited ſealing wax, any more than when it was preſented to an exhauſted and excited globe of glaſs.

HE provided himſelf, in like manner, with a globe of ſulphur, and another of roſin with a mixture of brick duſt, but the ſulphur could hardly be excited at all; whereas the roſin acted more powerfully than the ſealing wax had done. This he aſcribed to its being uſed while it was warm: for, in the ſame warm ſtate, it attracted leaf braſs without any attrition at all †.

HE ſays, that the excited roſin gave no light in the dark, and the ſulphur but little ‡.

WITH reſpect to the power of electricity in general, he obſerved, that a ſlight friction was ſufficient to excite it, and that a greater preſſure, or a more violent motion did not conſiderably increaſe it[*]. He ſays, that all the phenomena of electricity were improved by warmth; and diminiſhed by moiſture; which he attributed to the reſiſtance that the aqueous particles gave to the effluvia; and, like Mr. Boyle, and others before him, he was confirmed in this hypotheſis, by finding, that the interpoſition of linen cloth prevented any effects from being obſerved beyond it.

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* Phyſico-Mechanical Experiments, p. 65. † Ib. 154. ‡ Ib. 156.

He alſo obſerved, that when the tube was filled with other matter than air, as with dry writing ſand (which he actually tried) the attractive power of the effluvia was conſiderably abated; but he did not know what kind of bodies would produce that effect. He himſelf obſerves, that he found the electric virtue of a ſolid cylinder of glaſs to be, not indeed quite ſo ſtrong as that of a hollow tube, but more permanent[†].

That Mr. Hauksbee, after all, had no clear idea of the diſtinction of bodies into electrics and non-electrics, appears from ſome of his laſt experiments, in which he attempted to produce electric appearances from metals, and from the reaſons he gives for his want of ſucceſs in thoſe attempts. "From theſe "experiments," he ſays, "I may ſafely conclude, that if there "be any ſuch quality as light to be excited from a braſs body, "under the forementioned circumſtances," viz. of whirling and rubbing, "all the attrition of the ſeveral bodies I have uſed for "that purpoſe, have been too weak to force it from it. And "indeed, conſidering the cloſeneſs of the parts of metals, and "with what firmneſs they adhere, entangle, and attract one an- "other, a ſmall degree of attrition is not ſufficient to put their "parts into ſuch a motion as to produce an electrical quality, "which quality, under the forementioned circumſtances, I take "to be the appearance of light in ſuch a medium."

Considering what great ſucceſs Mr. Hauksbee had with his globe of glaſs, and his machine to give motion to it, it is ſurpriſing that the uſe of it ſhould have been ſo long diſcontinued after his death. To this circumſtance we may perhaps, in a great meaſure, aſcribe the ſlow progreſs that was afterwards made in electrical diſcoveries. Mr. Haukſbee's ſucceſſors confined themſelves to the uſe of tubes. I ſuppoſe becauſe they were lighter, more portable, and more eaſily managed in the experiments which they chiefly attended to: but the uſe of the globe would certainly have put them much ſooner in the way of making the capital diſcoveries, which were afterwards made in electricity.

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[*] Phyſico-Mechanical Experiments, p. 52.

[†] Ib. 64.

PERIOD III.

The experiments and discoveries of Mr. STEPHEN GREY, which were made prior to those of Monſieur du Faye, and which bring the hiſtory of Electricity to the year 1733.

NOTWITHSTANDING the important diſcoveries of Mr. Haukſbee, and the promiſing appearance they made, as an opening to farther diſcoveries, we find, after him, a great chaſm in the hiſtory of electricity, an interruption of diſcoveries, and, as far as we can learn, of experiments too, for the ſpace of near twenty years; and at a time when philoſophical knowledge of every other kind was making the moſt rapid progreſs, under the auſpices of the great Sir Iſaac Newton. But the attention of this great man happened to be engaged by other ſubjects, and this very circumſtance might be the reaſon why the attention of other philoſophers were alſo diverted from electricity.

After this long interval, commences a new æra in the hiſtory of electricity; in which we ſhall have the works of another labourer in this new field of philoſophy to contemplate, viz. Mr. Stephen Grey, a penſioner at the Charter Houſe. No perſon who ever applied to this ſtudy was more aſſiduous in making experiments, or had his heart more intirely in the work. This will appear by the prodigious number of experiments he made, and ſome conſiderable diſcoveries with which his perſeverance was crowned; as well as by the ſelf deceptions, to which his paſſionate fondneſs for new diſcoveries expoſed him.

BEFORE the year 1728, Mr. Stephen Grey had often obſerved, in electrical experiments made with a glaſs tube, and a down feather tied to the end of a ſmall ſtick, that, after its fibres had been drawn towards the tube, they would, upon the tube's being withdrawn, cling to the ſtick, as if it had been an electric body, or as if there had been ſome electricity communicated to the ſtick, or to the feather. This put him upon thinking, whether, if the feather were drawn through his fingers, it might not produce the ſame effect, by acquiring ſome degree of electricity. This experiment ſucceeded accordingly, upon his firſt trial; the ſmall downy fibres of the feather being attracted by his finger, when held near it; and ſometimes the upper part of the feather with its ſtem would be attracted alſo.

IT will be obvious to every electrician, that the ſucceſs of this experiment depended upon other principles, than thoſe to which he had a view in making it. Proceeding, however, in the ſame manner, he found the following ſubſtances to be all electric; *hair*, *ſilk*, *linen*, *woollen*, *paper*, *leather*, *wood*, *parchment*, and *ox gut* in which leaf gold had been beaten. He made all theſe ſubſtances very warm, and ſome of them quite hot before he rubbed them. He found light emitted in the dark by the ſilk and the linen, but more eſpecially by a piece of white *preſſing paper*, which is of the ſame nature with card paper. Not only did this ſubſtance, when made as hot as his fingers could bear, yield a light; but, when his fingers were held near it, a light iſſued from them alſo, attended with a crackling noiſe, like that produced by a glaſs tube, though not at ſo great a diſtance from the fingers *.

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* Philoſophical Tranſactions abridged, Vol. viii. p. 9.

THE preceding experiments bring us to the eve of a very conſiderable diſcovery in electricity, viz. the communication of that power from native electrics, to bodies, in which it is not capable of being excited; and alſo to a more accurate diſtinction of electrics from non-electrics. I ſhall relate the manner in which theſe important diſcoveries were made pretty fully, but, at the ſame time, as ſuccinctly as poſſible.

IN the month of February, 1729, Mr. Grey, after ſome fruitleſs attempts to make metals attractive, by heating, rubbing, and hammering, recollected a ſuſpicion which he had ſome years entertained; that, as a tube communicated its light to various bodies when it was rubbed in the dark, it might poſſibly, at the ſame time, communicate an electricity to them, by which had hitherto been underſtood only the power of attracting light bodies. For this purpoſe he provided himſelf with a tube three feet five inches long, and near one inch and two tenths in diameter; and to each end was fitted a cork, to keep the duſt out when the tube was not in uſe.

THE firſt experiments he made upon this occaſion were intended to try, if he could find any difference in its attraction when the tube was ſtopped at both ends by the corks, and when left entirely open; but he could perceive no ſenſible difference. It was, however, in the courſe of this experiment that, holding a down feather over againſt the upper end of the tube, he found that it would fly to the cork, being attracted and repelled by it, as well as by the tube itſelf. He then held the feather over againſt the flat end of the cork, and obſerved, that it was attracted and repelled many times together; at which, he ſays, he was much ſurpriſed, and concluded, that there was certainly an attractive virtue communicated to the cork by the excited tube.

HE then fixed an ivory ball upon a ſtick of fir, about four inches long; when, thruſting the other end into the cork, he found, that the ball attracted and repelled the feather, even with more vigour than the cork had done, repeating its attractions and repulſions many times ſucceſſively. He afterwards fixed the ball upon long ſticks, and upon pieces of braſs and iron wire, with the ſame ſucceſs; but he obſerved, that the feather was never ſo ſtrongly attracted by the wire, though it were held very near the tube, as by the ball at the end of it.

WHEN a wire of any conſiderable length was uſed, its vibrations, cauſed by the action of rubbing the tube, made it troubleſome to manage. This put Mr Grey upon thinking whether, if the ball were hung to a packthread, and ſuſpended by a loop on the tube, the electricity would not be carried down the line to the ball; and he found it to ſucceed according to his expectation. In this manner he ſuſpended various bodies to his tube, and found all of them to be capable of receiving electricity in the ſame manner.

AFTER trying theſe experiments with the longeſt light canes and reeds that he could conveniently uſe, he aſcended a balcony twenty-ſix feet high; and, faſtening a ſtring to his tube, he found, that the ball at the end of it would attract light bodies in the court below.

He then aſcended to greater heights, and by putting his long canes in the end of his tube, and faſtening a long ſtring to the end of the canes, he contrived to convey the electricity to much greater diſtances than he had done before; till, being able to carry it no farther perpendicularly, he next attempted to carry it horizontally; and from theſe attempts aroſe a diſcovery, of which he was not in the leaſt aware when he began them.

IN his firſt trial he made a loop at each end of a packthread, by means of which he ſuſpended it, at one end, on a nail driven into a beam, the other end hanging downwards. Through the loop which hung down, he put the line to which his ivory ball was faſtened, fixing the other end of it by a loop on his tube; ſo that one part of the line, along which the electricity was to be conveyed, viz. that to which the ball was faſtened, hung perpendicular, the reſt of the line lay horizontal. After this preparation, he put the leaf braſs under the ivory ball, and rubbed the tube, but not the leaſt ſign of attraction was perceived. Upon this he concluded, that when the electric virtue came to the loop of the packthread, which was ſuſpended on the beam, it went up the ſame to the beam; ſo that none, or very little, of it went down to the ball; and he could not, at that time, think of any method to prevent it.

On June the 30th 1729, Mr. Grey paid a viſit to Mr. Wheeler, to give him a ſpecimen of his experiments; when, after having made them from the greateſt heights which the houſe would admit, Mr. Wheeler was deſirous of trying whether they could not carry the electric virtue to a greater diſtance horizontally. Mr. Grey then told him of the fruitleſs attempt he had made to convey it in that direction: upon which Mr. Wheeler propoſed to ſuſpend the line to be electrified by another of *ſilk*, inſtead of *packthread*; and Mr. Grey told him, it might do better, on account of its ſmallneſs; as leſs of the virtue would probably paſs off by it than had done by the thick hempen line, which he had uſed before. With this expedient, they ſucceeded far beyond their expectations.

THE firſt experiment they made after this expedient occurred to them, was in a matted gallery at Mr. Wheeler's houſe, July 2d 1729, about ten o'clock in the morning, as Mr. Grey, after his uſual manner, has minutely recorded it. About four feet from the end of the gallery, they faſtened a line acroſs the place. The middle part of this line was ſilk, the reſt packthread. They then laid the line to which the ivory ball was hung, by which the electric virtue was to be conveyed to it from the tube, and which was eighty feet and a half in length, acroſs this ſilken line, ſo that the ball hung about nine feet below it. The other end of the line was, by a loop, faſtened to the tube, which they excited at the other end of the room. After this preparation, they put the leaf braſs under the ivory ball, and, upon rubbing the tube, it was attracted, and kept ſuſpended for ſome time.

THE gallery not permitting them to go any greater lengths with a ſingle line of communication, they contrived to return the line, making the whole length of it almoſt twice that of the gallery, or about one hundred and forty-ſeven feet, which anſwered very well. But, ſuſpecting that the attraction would be ſtronger without doubling or returning the line, they made uſe of a line one hundred and twenty-four feet long, running in one direction in the barn; and, as they expected, they found the attraction ſtronger than when the line had been returned in the gallery.

JULY the 3d, proceeding to make more returns of the line, the ſilk which ſupported it happened to break, not being able to bear the weight of it, when ſhaken with the motion that was given to it by rubbing the tube. Upon this they endeavoured to ſupport it by a ſmall iron wire, inſtead of the ſilken ſtring; but this alſo breaking, they made uſe of a braſs wire a little thicker. But this braſs wire, though it ſupported the line of communication very well, did not anſwer the purpoſe of theſe young electricians: for, upon rubbing the tube, no electricity was perceived at the end of the line. It had all gone off by the braſs wire which ſupported it. They had recourſe to braſs wires, as being ſtronger than their ſilken lines, and no thicker; for the ſame reaſon that they had before uſed ſilken lines in preference to hempen ſtrings; becauſe they could have them ſtronger, and at the ſame time ſmaller. But the reſult of this experiment convinced them, that the ſucceſs of it depended upon their ſupporting lines being *ſilk*, and not, as they had imagined, upon their being *ſmall*. For the electric virtue went off as effectually by the ſmall braſs wire, as it had done by the thick hempen cord.

Being obliged, therefore, to return to their ſilken lines, they contrived them to ſupport very great lengths of the hempen line of communication; and actually conveyed the electric virtue ſeven hundred and ſixty-five feet, nor did they perceive that the effect was ſenſibly diminiſhed by the diſtance*.

In the ſame manner in which *ſilk* was found to be a non-conductor, it is probable that, about the ſame time, *hair*, *roſin*, *glaſs*, and perhaps ſome other electric ſubſtances were found to have the ſame property, though the diſcovery be no where particularly mentioned: for we ſhall preſently find Mr. Grey making uſe of them, to inſulate the bodies which he electrified.

After this, Mr. Grey and his friend amuſed themſelves with trying how *large* ſurfaces might be impregnated with the electric effluvia; electrifying a large map, table cloth, &c. They alſo carried the electric virtue ſeveral ways at the ſame time, and to a conſiderable diſtance each way.

The magnetic effluvia, they found did not in the leaſt interfere with the electric; for when they had electrified the load ſtone, with a key hanging to it, they both attracted leaf braſs like other ſubſtances.

Some time after this, Mr. Wheeler, in the abſence of Mr. Grey, electrified a red hot poker, and found the attraction to be the ſame as when it was cold. He alſo ſuſpended a live chicken upon the tube by the legs, and found the breaſt of it ſtrongly electrical †.

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* Phil. Tranſ. abridged, Vol. vii. p. 15.

† Ib. p. 16.

IN Auguſt, 1729, Mr. Grey advanced one ſtep farther in his electrical operations. He found that he could convey the electric virtue from the tube to the line of communication without touching it, and that holding the excited tube near it was ſufficient. Repeating his former experiments with this variation, in conjunction with Mr. Wheeler, and among others, carrying the electric virtue ſeveral ways at the ſame time without touching the line, they always obſerved, that the attraction was ſtrongeſt at the place which was moſt remote from the tube; a fact which they might have obſerved, if they had attended to it, in their former experiments *.

SOME time in the ſame month, Mr. Wheeler and Mr. Grey in conjunction made ſome experiments, in order to try whether the electric attraction was in proportion to the quantity of matter in bodies; and with this view they electrified a ſolid cube of oak, and another of the ſame dimenſions which was hollow; but they could not perceive any difference in their attractive power; though it was Mr. Grey's opinion, that the electric effluvia paſſed through all the parts of the ſolid cube †.

ON the 13th of Auguſt in the ſame year, Mr. Grey made another improvement in his electrical apparatus, by finding that he could electrify a *rod*, as well as a *thread*, without inſerting any part of it into his excited tube. He took a large pole twenty-ſeven feet long, two inches and one half in diameter at one end, and one inch and one half at the other. It was a ſort of wood which is called horſe-beach, and had its rind on. This pole he ſuſpended horizontally by hair lines, and at the ſmall end of the pole he hung a cork, by means of a packthread about one foot long, and put a ſmall leaden ball upon the cork, to keep the packthread extended. Then the leaf braſs being put under the cork, the tube rubbed, and held near the larger end of the pole, the cork ball at the oppoſite end attracted the leaf braſs ſtrongly, to the height of an inch or more. Mr. Grey alſo obſerved, that though the leaf braſs was attracted by any part of the pole, it was not near ſo ſtrongly as by the cork *.

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* Phil. Tranſ. abridged, Vol. vii. p. 17.

† Ibid. p. 17.

ABOUT the beginning of September, Mr. Grey made experiments, to ſhew that the electric effluvia might be carried in a circle, as well as along lines, and be communicated from one circle to another; and alſo that it might be done whether the circles were vertical, or horizontal.

ABOUT the latter end of autumn, or the beginning of winter 1729, Mr. Grey reſumed his inquiries after other electrical bodies, and found many more to have the ſame property, but he mentions only the dry leaves of ſeveral trees; from whence he concluded, that the leaves of all vegetables had that attractive virtue †.

WE are now advanced to a new ſcene of Mr. Grey's electrical experiments, viz. upon *fluids*, and upon *animal bodies*. Having no other method of trying whether any ſubſtances could have the electric virtue communicated to them, but by making them raiſe light bodies placed upon a ſtand under them, it may eaſily be imagined, that he could not well contrive to put a fluid body into that ſituation. The only thing that Mr. Grey could do in this way, was to make uſe of a bubble, in which form a fluid is capable of being held in a ſtate of ſuſpenſion. Accordingly on March 23d and 25th, 1730, he diſſolved ſoap in Thames water, and ſuſpending a tobacco pipe, he blew a bubble at the head of it; and, bringing the excited tube near the ſmall end, he found the bubble to attract leaf braſs to the height of two, and of four inches ‡.

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* Phil. Tranſ. abridged, Vol. vii. p. 18.

† Ibid p. 19.

‡ Ibid.

APRIL the 8th, 1730, Mr. Grey ſuſpended a boy on hair lines in a horizontal poſition, juſt as all electricians had, before, been uſed to ſuſpend their hempen lines of communication, and their wooden rods; then, bringing the excited tube near his feet, he found that the leaf braſs was attracted by his head with much vigour, ſo as to riſe to the height of eight, and ſometimes of ten inches. When the leaf braſs was put under his feet, and the tube brought near his head, the attraction was ſmall; and when the leaf braſs was brought under his head, and the tube held over it, there was no attraction at all. Mr. Grey does not attempt to aſſign any reaſon for theſe appearances. It was not till many years after this time, that the influence of *points* in receiving and emitting the electric effluvia was obſerved. While the boy was ſuſpended, Mr. Grey amuſed himſelf with making the electricity operate in ſeveral parts of his body, at the ſame time; and at the end of long rods, which he made him hold in his hands, and in diverſifying the experiment ſeveral other ways *.

IT is curious to obſerve the inference which Mr. Grey makes from theſe experiments. By them, ſays he, we ſee, that animals receive a greater quantity of electric fluid than other bodies; and that it may be conveyed from them ſeveral ways at the ſame time, to conſiderable diſtances. He had no idea that the bodies of animals receive electricity only by means of the moiſture that is in them, and that this hempen line of communication, and his wooden rods could not have been electrified at all, if they had been perfectly dry.

IN all theſe experiments Mr. Grey obſerved, that the leaf braſs was attracted to a much greater height from the top of a narrow ſtand than from the table; and, at leaſt, three times higher than when it was laid on the floor of the room.

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* Phil. Tranſ. abridged, Vol. vii. p. 20.

About this time Mr. Grey communicated to the Royal Society his ſuſpicion, that bodies attracted more or leſs according to their *colour*, though the ſubſtance was the ſame, and the weight and ſize equal. He ſays, he found red, orange, and yellow attracted at leaſt three or four times ſtronger than green, blue, or purple; but he forbore communicating a more particular account of them, till he had tried a more accurate method which, he ſays he had thought of, to make the experiments. The communication, however, was never made. The thing itſelf was only a deception, as will be ſhewn in ſome ſubſequent experiments made by Monſieur du Fay *.

Mr. Grey, having found that he could communicate electricity to a bubble of ſoap and water, was encouraged to attempt communicating it to water itſelf. In order to this, he electrified a wooden diſh full of water, placed on a cake of roſin, or a pane of glaſs, and obſerved, that if a ſmall piece of thread, a narrow ſlip of thin paper, or a piece of ſheet glaſs was held over the water, in an horizontal poſition, at the diſtance of an inch or ſomething more, they would be attracted to the ſurface of the water and then repelled; but he imagined that theſe attractions and repulſions were not repeated ſo often as they would have been, if the body had been ſolid.

But he afterwards contrived to ſhew the effect of electricity upon water in another and more effectual manner. As this experiment was very curious and exhibited an appearance which was quite new to the electricians of thoſe times, I ſhall relate the particulars of it very fully, and generally in Mr. Grey's own words †.

He filled a ſmall cup with water higher than the brim, and when he had held an excited tube over it, at the diſtance of about an inch or more, he ſays, that if it were a large tube, there would firſt ariſe a little mountain of water from the top of it, of a conical form; from the vertex of which there proceeded a light, very viſible when the experiment was performed in a dark room, and a ſnapping noiſe, almoſt like that which was made when the finger was held near the tube, but not quite ſo loud, and of a more flat ſound. Upon this, ſays he, immediately the mountain, if I may ſo call it, falls into the reſt of the water, and puts it into a tremulous and waving motion.

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* Phil. Tranſ. abridged, Vol. vii. p. 22.

† Ibid. p. 23.

WHEN he repeated this experiment in the ſun ſhine, he perceived that very ſmall particles of water were thrown from the top of the mountain; and that, ſometimes, there would ariſe a fine ſtream of water from the vertex of the cone, in the manner of a fountain, from which there iſſued a fine ſteam or vapour, whoſe particles were ſo ſmall as not to be ſeen; yet, he ſays, he was certain it muſt be ſo; ſince the under ſide of the tube was wet, as he found when he came to rub it afterwards. He adds, that he had ſince found, that though there does not always ariſe that cylinder of water, yet that there is always a ſtream of inviſible particles thrown on the tube, and ſometimes to that degree as to be viſible on it.

WHEN ſome of the larger cups were uſed (his ſizes were from three fourths to one tenth of an inch in diameter) which, he ſays were to be filled as high as could be done without running over; the middle part of the ſurface, which was flat, would be depreſſed, upon the approach of the tube, into a concave, and the parts towards the edge be raiſed; and that when the tube was held over againſt the ſide of the water, little conical protuberances of water iſſued out from it horizontally; and, after the crackling noiſe, returned to the reſt of the water; and that ſometimes ſmall particles would be thrown off from it, as from the ſmall portions of it above mentioned.

This laſt experiment he repeated with hot water, and found that it was attracted much more ſtrongly, and at a much greater diſtance than before. The ſteam ariſing from the vertex was in this caſe viſible, and the tube was ſprinkled with large drops of water.

He tried theſe experiments, in the ſame manner, with quickſilver, which was likewiſe raiſed up, but, by reaſon of its gravity, not to ſo great a height as the water: but, he ſays, that the ſnapping noiſe was louder, and laſted much longer than it did with the water *.

It is not eaſy to know what to make of the next ſet of experiments which engaged the attention of Mr. Grey, or how far he deceived himſelf in the reſults of them. He fancied that he had diſcovered a *perpetual attractive power* in all electric bodies which did not require heating, rubbing, or any kind of attrition to be excited. The following experiments, he imagined proved the diſcovery.

He took nineteen different ſubſtances, which were either roſin, gum lac, ſhell lac, bees wax, ſulphur, pitch, or two or three of theſe differently compounded. Theſe he melted in a ſpherical iron ladle, except the ſulphur, which was beſt done in a glaſs veſſel. When theſe were taken out of the ladle, and their ſpherical ſurfaces hardened, he ſays, they would not attract till the heat was abated, or till they came to a certain degree of warmth; that then there was a ſmall attraction, which increaſed till the ſubſtance was cold, when it was very conſiderable †.

The manner in which he preſerved theſe ſubſtances in a ſtate of attraction, was by wrapping them in any thing which would keep them from the external air. At firſt, for the ſmaller bodies, he uſed white paper, and for the larger ones, white flannel;

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* Phil. Tranſ. abridged, Vol. vii p. 24.

† Ibid.

but he afterwards found that black worſted ſtockings would do as well. Being thus cloathed, he put them into a large firm box; where they remained, till he had occaſion to make uſe of them.

HE obſerved theſe bodies for thirty days, and found that they continued to act as vigorouſly as on the firſt or ſecond day; and that they retained their power till the time of his writing, when ſome of them had been prepared above four months.

HE makes the moſt particular mention of a large cone of ſtone ſulphur, covered with a drinking glaſs in which it was made; and ſays, that whenever the glaſs was taken off, it would attract as ſtrongly as the ſulphur, which was kept covered in the box. In fair weather, the glaſs would attract alſo, but not ſo ſtrongly as the ſulphur, which never failed to attract, let the wind or the weather be ever ſo variable; as would all the other bodies, only in wet weather, the attraction was not ſo great as in fair weather.

HE alſo mentions a cake of melted ſulphur, which he kept without any cover, in the ſame place with the body abovementioned, and where the ſun did not ſhine upon them; and ſays, that it continued to attract till the time of his writing; but that its attraction was not one tenth part of that of the cone of ſulphur which was covered.

THESE attractions he tried by a fine thread hanging from the end of a ſtick. He held the electric body in one hand, and the ſtick in the other; and could perceive the attraction at as great a diſtance as he could hold them.

AT the time of his writing, he was upon the ſubject of permanent electricity in glaſs, but had not then completed his experiments*.

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* Phil. Tranſ. abridged, Vol. vi. p. 27.

GREAT light will be thrown upon theſe experiments of Mr. Grey by ſome that will be hereafter related of Mr. Wilcke. It is probable that the glaſs veſſel in theſe experiments was poſſeſſed of one electricity, and the ſulphur, &c. of the other. But the two electricities were not diſcovered till afterwards.

WE are now come to a different ſet of electrical experiments, made by Mr. Grey and Mr. Wheeler in conjunction, ſimilar to ſome of Mr. Haukſbee's.

IN the firſt place, Mr. Grey made ſome experiments, which, probably unknown to him, had been made before by Mr. Boyle, on excited glaſs, and ſeveral other bodies *in vacuo*, and found that they would attract at very near the ſame diſtance as *in pleno*. To determine this, he ſuſpended the excited ſubſtance in a receiver of an air-pump, and when it was exhauſted, he let the electric down to a proper diſtance from ſome light bodies, placed on a ſtand below. The event was, as near as could be judged, the ſame in vacuo as in the open air, if the experiment was made in the ſame receiver, and if the electric was brought to the light bodies at the ſame diſtance of time from the act of excitation *.

ABOUT the latter end of Auguſt 1732, Mr. Grey and Mr. Wheeler ſuſpended, from the top of a receiver, a white thread, which hung down to the middle of it. Then exhauſting the receiver, and rubbing it, the thread was attracted vigorouſly. When it was at reſt, and hung perpendicularly, the excited tube attracted it; and when the tube was taken away ſlowly, the thread returned to its perpendicular ſituation; but the tube being removed haſtily, the thread jumped to the oppoſite ſide of the receiver. This laſt effect followed, if the hand was haſtily removed from the receiver; and at firſt it appeared, in both caſes, unaccountable to them; but upon farther conſideration, they concluded, that it proceeded from the motion of the air, made by the tube, or the hand, which ſhook off the attraction on that ſide, and not on the other *. They alſo found, that an excited tube would attract the thread through another receiver, which was put over that in which it was ſuſpended. And, ſome time after, Mr. Wheeler found that the thread was attracted through five receivers put one over another, and all exhauſted: he even thought that, in this caſe, the attraction was rather ſtronger than when a ſingle receiver was uſed. N. B. The more effectually to keep any thing of moiſture out of the receivers, which would have been of bad conſequence in this experiment, inſtead of wet leather, he made uſe of a cement made of wax and turpentine, which Mr. Boyle uſed in his experiments †.

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* Phil. Tranſ. abridged, Vol. vi. p. 27.

THESE two gentlemen, about the ſame time, made a curious experiment which, they ſay, ſhewed that attraction is communicated thro' opaque, as well as tranſparent bodies, not in vacuo. But a little knowledge of metal, as a conductor of electricity, would have ſaved them the trouble they gave themſelves. They took a large hand-bell, and taking out the clapper, they ſuſpended a cork, beſmeared with honey, from the top of it; and ſet it on a piece of glaſs, on which they had put ſome leaf braſs. The excited tube was then brought near ſeveral parts of the bell; and, upon taking it up, ſeveral pieces of leaf braſs were found ſticking to the cork, and others were removed from the places in which they had been left, having, probably, been attracted by the bell ‡.

WE ſee by how ſmall ſteps advances were made in the ſcience of electricity, by ſome experiments made by Mr. Grey, 16th June 1731, and which he has thought worth recording; though they

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* Phil. Tranſ. abridged, Vol. vii. p. 56.

† Ibid. p. 97.

‡ Ibid. p. 96.

contain hardly any thing which we ſhould think new, notwithſtanding the diſcoveries appeared pretty conſiderable to him.

HE electrified a boy ſtanding on cakes of roſin, as ſtrongly as he had before electrified him when ſuſpended on hair lines. He afterwards electrified a boy ſuſpended on hair lines, by means of a line of communication from another boy who was electrified, at ſome feet diſtance from him. He varied this experiment with rods and boys ſeveral ways; and concluded from it, that the electric virtue might not only be carried from the tube, by a rod, or line, to diſtant bodies, but that the ſame rod, or line, would communicate that virtue to another rod, or line, at a diſtance from it; and that, by this other rod, or line, the attractive force might be carried to ſtill more diſtant bodies. This experiment ſhews that Mr. Grey had not properly conſidered the line of communication, and the body electrified by it, as one and the ſame thing, in an electrical view, differing only in form, as they were both alike conductors of electricity.

IN December following, Mr. Grey carried this experiment ſomething farther, by conveying electricity to bodies which did not touch the line of communication, making it paſs through the center of hoops ſtanding on glaſs. One of his hoops was twenty, another forty inches in diameter *.

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* Phil. Tranſ. abridged, Vol. vii. p. 100.

PERIOD IV.

THE EXPERIMENTS AND DISCOVERIES OF MR. DU FAY.

HITHERTO the ſpirit of electricity ſeems to have been confined to England; but, about this time, we find that it had paſſed the ſeas, and that ingenious foreigners were ambitious of diſtinguiſhing themſelves, and acquiring reputation in this new field of glory. Mr. Du Fay, intendant of the French king's gardens, and member of the academy of ſciences at Paris, aſſiduouſly repeated the experiments above-mentioned of Mr. Grey, and likewiſe added to the common ſtock many new ones of his own. To him we are alſo indebted for the obſervation of ſeveral general properties of electricity, or rules concerning the method of its action, which had not been taken notice of before, and which reduced to fewer propoſitions what had been diſcovered concerning it. Theſe experiments were compriſed in eight memoirs, inſerted in the Hiſtory of the Academy of Sciences for the years 1733, 1734, and 1737; and an account of ſome of them alſo makes an article in the Philoſophical Tranſactions, dated December 27, 1733. The firſt of his memoirs contain a hiſtory of electricity, brought down to the year 1732[*].

HE found that all bodies, except metallic, ſoft, and fluid ones, might be made electric, by firſt heating them, more or leſs, and then rubbing them on any ſort of cloth. He alſo excepts thoſe ſubſtances which grow ſoft by heat, as gum; or which diſſolve in water, as glue. He alſo remarked, that the hardeſt ſtone and marble required more chafing and heating than other bodies, and that the ſame rule obtains with regard to woods; ſo that box, lignum vitæ, and other kinds of very hard wood muſt be chafed almoſt to a degree of burning; whereas fir, lime-tree, and cork require but a moderate heat *. Among the more perfect electrics he enumerates all *vitrifications*, the Venetian and Muſcovite *talc*, the *phoſphorus of Berne*, *gypſum*, *tranſparent ſelenites*, and in general all *tranſparent ſtones*, of whatſoever kind †. Of *ſalts* he tried only *alum*, and *ſugar candy*, both of which he found to be electrical, after warming and rubbing; and he ſuppoſed that all the reſt would be found to have the ſame properties, if due precautions were obſerved in making the experiments. He alſo obſerved the electricity of all kinds of *hair*, *ſilk*, *wool*, and *cotton*, and eſpecially, what appeared to him very extraordinary, the powerful electricity of the *back of a dog*, and ſtill more that of a *cat* ‡.

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[*] Dantzick Memoirs, Vol. i. p. 195.

He found that not only damp air, but alſo great heat was prejudicial to electricity, and that his experiments often failed in the warmeſt hours of a moderately hot day §. Alſo when he heated ſeveral bodies, in order to excite their electric power, he found that in ſome the hotteſt, and in others the cooleſt parts only were electrical ||.

He ſays, that, purſuing Mr. Grey's experiments, to make water receive electricity, he found, that all bodies, without exception, whether ſolid or fluid, were capable of it, when they were placed on glaſs, or ſealing-wax ſlightly warmed, or only dried, and the excited tube was brought near them. He particularly

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* Phil. Tranſ. abridged, Vol. viii. p. 393.

† Ac. Par. 1733. M. p. 105.

‡ Ac. Par. 1733, M. p. 107.

§ Dantzick Memoirs, Vol. i. p. 211.

|| Ac. Par. 1733, M. p. 109.

larly mentions his having made the experiments with ice, lighted wood, red hot iron *, coal, and every thing that happened to be at hand at the time; and to his great ſurprize, remarked, that ſuch bodies as were of themſelves the leaſt electric, had the greateſt degree of electricity communicated to them by the approach of the excited tube †.

To determine whether the tranſparency of glaſs was the cauſe of the tranſmiſſion of the electric effluvia through that ſubſtance (for no one at that time ſuſpected that bodies could be affected by electricity through glaſs in conſequence of its being actually permeable to the electric matter) he uſed ſealing wax, and found that light bodies were affected through that opaque ſubſtance, as much as through glaſs ‡.

HE refutes Mr. Grey's aſſertion concerning the different electricity of differently coloured bodies, and ſhews that it proceeded not from the colour as a *colour*, but from the ſubſtance which was employed in dying it §.

IN order to determine whether ſimple *colour* had any influence in electricity, he introduced a beam of the ſun's light into a darkened room, and made his experiments on bodies illuminated with the different primitive colours; and he found that, in no reſpect whatever, did the different colours make any difference in the power of receiving, communicating, or deſtroying electricity ‖.

HAVING communicated the electricity of the tube, by means of a packthread, after Mr. Grey's manner, he obſerved, that the experiment ſucceeded better for wetting the line; and though he made the experiment at the diſtance of one thouſand two hundred and fifty-ſix feet, when the wind was high, the line making eight returns, and paſſing through two different walks of a

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* Nollet's Recherches, p. 212.

† Ac. Par. 1733. M. p. 115.

‡ Ac. Par. 1737, M. p. 338.

§ Ibid. 1733. M. p. 337.

‖ Ibid. p. 334.

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garden, that the electric virtue was ſtill communicated *. He alſo made uſe of glaſs tubes, ſometimes lined with ſealing wax, and found that it anſwered as well as ſilk lines, and was often more conveniently applied †.

OUR ingenious electrician varied this experiment by ſuſpending two cords with their ends oppoſite to one another, and found that when they were placed at no greater diſtance than an inch, the electricity was communicated without any ſenſible diminution from the one to the other; but at the diſtance of a foot it was hardly ſenſible. When the cords were placed at the diſtance of three inches, he found that a lighted candle held between them did not prevent the tranſmiſſion of electricity, nor did the blaſt of a pair of bellows ‡. Bringing wood and other conducting ſubſtances, ſuſpended by ſilk, to this electrified cord, he found, as he had concluded *a priori,* that it loſt only part of its electricity; the whole quantity being equally diſtributed between them §.

THE electric ſpark from a living body, which makes a principal part of the diverſion of gentlemen and ladies, who come to ſee experiments in electricity, was firſt obſerved by Mr. du Fay, accompanied at that time, as in moſt of his experiments, by the Abbé Nollet, who, afterwards, we ſhall find, did himſelf obtain a diſtinguiſhed name among electricians.

MR. DU FAY, having got himſelf ſuſpended on ſilk lines, as Mr. Grey had done the child mentioned above, obſerved, that, as ſoon as he was electrified, if another perſon approached him, and brought his hand within an inch, or thereabouts, of his face, legs, hands, or cloaths, there immediately iſſued from his body one or more pricking ſhoots, attended with a crackling noiſe. He ſays this experiment occaſioned to the perſon who brought his hand near him, as well as to himſelf, a little pain, reſembling that of the ſudden prick of a pin, or the burning from a ſpark of fire; and that it was felt as ſenſibly through his cloaths, as on his bare face, or hands. He alſo obſerves, that, in the dark, thoſe ſnappings were ſo many ſparks of fire*.

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* Phil. Tranſ. abridged, Vol. viii. p. 395. † Ac. Par. 1733. M. p. 345.
‡ Ac. Par. 1733. M. p. 350. § Ibid. p. 352.

THE Abbé Nollet ſays he ſhall never forget the ſurprize, which the firſt electrical ſpark which was ever drawn from the human body excited, both in Mr. du Fay, and in himſelf†.

HE ſays that thoſe ſnappings and ſparks were not excited, if a bit of wood, of cloth, or of any other ſubſtance than a living human body, was brought near him; except metal, which produced very nearly the ſame effect as the human body. He was not aware, that it was owing to points, or partial dryneſs, in the ſubſtances which he mentions, that they did not take a full and ſtrong ſpark. He ſeems alſo to have been under ſome deception, when he imagined that the fleſh of dead animals gave only an uniform light, without any ſnapping, or ſparks ‡.

FROM this circumſtance, however, he, at that time concluded, that the bodies of living animals, (and alſo metals) were ſurrounded with an atmoſphere of vapour, which was actually ſet on fire by electric light §.

HE obſerved, that a cat emitted an electric ſpark, which evidently gave her pain, when the finger was brought to any part of her body, after ſhe had been ſtroked, while ſhe was ſitting on a ſilk cuſhion. This muſt have appeared very extraordinary, before it was known, that the electric matter paſſed from the hand to the cat, in the act of rubbing ||.

WITH the electric ſparks, he imagined he could have fired inflammable ſubſtances; and he made ſeveral attempts with tinder

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* Phil. Tranſ. Vol. viii. p. 395. Ac. Par. 1733. M. p. 353.

† Leçons de Phyſique, Vol. vi. p. 408.

‡ Phil. Tranſ. abridged, Vol. viii. p. 395. Ac. Par. 1734. M. 714.

§ Dantzick Memoirs, Vol. i. p. 215, 230.

|| Ibid. p. 216.

and gunpowder, but without ſucceſs: he found no appearance of real fire. This was a diſcovery reſerved for the Germans *.

THE two next capital obſervations of Mr. Du Fay I ſhall repeat in his own words, becauſe they are important and curious; and yet the former of them is little more than what Otto Guericke had obſerved before him. "I diſcovered," he ſays, "a very ſimple principle, which accounts for a great part of the irregularities, and, if I may uſe the term, of the caprices, that ſeem to accompany moſt of the experiments in electricity. This principle is, that electric bodies attract all thoſe which are not ſo, and repel them as ſoon as they are become electric, by the vicinity or contact of the electric body. Thus leaf gold is firſt attracted by the tube; acquires electricity by approaching it, and, conſequently, is immediately repelled by it; nor is it re-attracted, while it retains its electric quality. But if, while it is thus ſuſtained in the air, it chance to light on ſome other body, it ſtraightway loſes its electricity, and conſequently is re-attracted by the tube; which, after having given it a new electricity, repels it a ſecond time; and this repulſion continues as long as the tube keeps its power. Upon applying this principle to various experiments of electricity, one will be ſurpriſed at the number of obſcure and puzzling facts which it clears up." By the help of this principle, he, particularly, endeavours to explain ſeveral of Mr. Haukſbee's experiments †.

"CHANCE, he ſays, has thrown in my way another principle more univerſal and remarkable than the preceding one; and which caſts a new light upon the ſubject of electricity. The principle is, that there are *two kinds of electricity*, very different from one another; one of which I call *vitreous*, the other

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* Dantzick Memoirs, Vol. i. p. 229.

† Phil. Tranſ. abridged, Vol. viii. p. 396.

" *reſinous* electricity. The firſt is that of glaſs, rock-cryſtal, precious ſtones, hair of animals, wool, and many other bodies. " The ſecond is that of amber, copal, gum lac, ſilk, thread, " paper, and a vaſt number of other ſubſtances. The characteriſtics of theſe two electricities are, that they repel themſelves, " and attract each other. Thus a body of the vitreous electricity repels all other bodies poſſeſſed of the vitreous, and on the " contrary, attracts all thoſe of the reſinous electricity. The " reſinous, alſo, repels the reſinous, and attracts the vitreous. " From this principle, one may eaſily deduce the explanation " of a great number of other phenomena; and it is probable, " that this truth will lead us to the diſcovery of many other " things."

THIS very capital diſcovery was, as the ingenious author acknowledges, perfectly accidental, having been made in conſequence of caſually obſerving (which, he ſays, was to his great ſurprize) that, having cauſed a piece of leaf gold to be repelled, and ſuſpended in the air, by an excited glaſs tube, and meaning likewiſe to chaſe it about the room by a piece of excited gum copal, inſtead of being repelled by it, as it was by the glaſs tube, it was eagerly attracted. The ſame was the caſe with ſealing-wax, and the other ſubſtances enumerated above. He alſo obſerved, that when a piece of leaf gold was electrified by excited ſealing-wax, &c. it was conſtantly attracted by excited glaſs, but repelled by excited ſealing-wax, &c. *

OUR ingenious electrician was, however, too haſty in concluding, as he did, that the two electricities which he had diſcovered were altogther independent of the ſubſtance with which the electrics were rubbed. All the difference, he ſays, produced by a change of the rubber, was that of *more or leſs* of the ſame

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[*] Ac. Par. 1733, M. p. 627.

kind *. We ſhall find that, in a much later period, the contrary was diſcovered to be true by Mr. Canton.

IN order to know, immediately, to which of the two claſſes of electricity any body belonged, he made a ſilk thread electrical, and brought it to the body, when it was excited. If it repelled the thread, he concluded it was of the ſame electricity with it, viz. reſinous; if it attracted it, he concluded it was vitreous †. He had alſo other ingenious methods to aſcertain the ſame thing ‡.

HE alſo obſerved, that communicated electricity had the ſame property as the excited. For having electrified, by the glaſs tube, balls of wood or ivory; he found them to repel the bodies which the tube repelled, and to attract thoſe which the tube attracted. If they had the reſinous electricity communicated to them, they obſerved the ſame rule, by attracting thoſe bodies which had the vitreous electricity communicated to them, and repelling thoſe which had received the reſinous. But, he obſerves, the experiment would not ſucceed, if the bodies were not made equally electrical; for, if one of them was weakly electrical, it would be attracted by that which was much more ſtrongly electrical, of whatever quality it was.

THIS diſcovery of the two electricities was certainly a capital one, but was, notwithſtanding, left very imperfect by Mr. Du Fay. We ſhall ſee that Dr. Franklin found, that, in all probability, the vitreous electricity is poſitive, or a redundancy of electric matter; and the reſinous, negative, or a want of it; and that Mr. Canton has diſcovered, that it depends upon the ſurface of the electric bodies, and of the rubber, whether the electricity be poſitive or negative.

THE doctrine of two different electricities, produced by exciting different ſubſtances, conſiderable as the diſcovery of it was,

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* Ac. Par. 1733, M. p. 639.

† Phil. Tranſ. abridged, Vol. viii. p. 397.

‡ Ac. Par. 1733, M. p. 639.

ſeems to have been dropped after Mr. Du Fay, and thoſe effects aſcribed to other cauſes; which is an inſtance that ſcience ſometimes goes backwards.

MR. DU FAY himſelf ſeems, at laſt, to have adopted the opinion, which generally prevailed to the time of Dr. Franklin; that the two electricities differed only in degree, and that the ſtronger attracted the weaker: not conſidering that, upon this principle, bodies poſſeſſed of the two electricities ought to attract one another leſs forcibly, than if one of them had not been electrified at all, which is contrary to fact.

IT will be ſeen that, many years after, Mr. Kinnerſley of Philadelphia, a friend of Dr. Franklin's, being at Boſton in New England, made ſome experiments which again ſhewed the difference of the two electricities. He communicated thoſe experiments to Dr. Franklin, who repeated and explained them [*].

MR. DU FAY was the firſt perſon who endeavoured to excite a tube in which air was condenſed, and, to his great ſurprize, found the attempt ineffectual. Suſpecting this might be owing to moiſture, which he might have forced into the tube, in uſing his condenſing inſtrument, he cemented a large copper eolipile to his tube, and compreſſed the air in it, by putting the eolipile upon the fire. After this, he turned a cock, which he had placed to prevent the return of the compreſſed air, and diſengaged the tube from the eolipile; but he ſtill found the excitation to be impoſſible [†]. The Abbé Nollet, who aſſiſted at moſt of this gentleman's experiments, declares himſelf not ſatisfied even with this precaution; thinking that the non-excitation of the tube might ſtill be owing to the moiſture, which always exiſts in the air, and the particles of which muſt be drawn nearer together by condenſation [‡]. In anſwer to this objection, Mr.

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[*] See his Letters.

[†] Ac. Par. 1734. M. p. 489.

[‡] Nollet's Recherches, p. 258.

Boulanger ſays, that a ſmall glaſs full of water poured into a tube, and immediately thrown out again, will not deſtroy the excitability of the glaſs near ſo much as the condenſed air *.

Mr. Du Fay found no difference in the excitation of a glaſs tube whether it was filled with warm *ſand* or not; but when the tube was cool it was not ſo eaſily excited. The excitation was more obſtructed by *bran*, and much more by *water*, warm or cold; though, he ſays, that the electricity was not quite deſtroyed by it †.

Mr. Du Fay took a good deal of pains to aſcertain the effect of electricity *in vacuo*, but his concluſion can hardly be depended upon. For, he ſays, that *glaſs*, and other ſubſtances poſſeſſed of the ſame kind of electricity, are hardly capable of being excited *in vacuo*; whereas *amber*, and the ſubſtances of that claſs, are excited as eaſily, and as vigorouſly *in vacuo*, as in the open air. The *vacuum* he made ſeems to have been as good as can be made by the better kind of our air-pumps ‡.

This philoſopher was the firſt who obſerved that electric ſubſtances attract the dew more than conductors. He obſerved that a glaſs veſſel, placed on a metal cup, and ſet in the open air all day, will often be wet when the metal is dry. S. Beccaria accounts for this fact, by ſuppoſing that alterations in the electricity of the air eaſily produce correſpondent alterations in the electricity of metals, in which the electric fluid moves with the utmoſt eaſe, but not in glaſs. Whenever, therefore, the ſtate of the electric fluid in the air is altered, the glaſs is electrified *plus* or *minus*, and therefore attracts the vapours in the air §.

It muſt be obſerved, that Mr. Granville Wheeler, in the autumn of the year 1732, made ſeveral curious experiments, relating to the repulſive force of electricity. Theſe he repeated to Mr. Grey in the ſummer following, and deſigned to communicate them, through his hands, to the Royal Society; but, deferring the execution of it from time to time, he was informed that Mr. Du Fay had taken notice of the ſame ſolution of the repulſive force. Upon this he laid aſide all thoughts of communicating his diſcovery to the public: but, finding that his experiments were different from thoſe of Mr. Du Fay, he was perſuaded to publiſh them in the Philoſophical Tranſactions for the year 1739.

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* Boulanger, p. 132.

† Ac. Par. 1733. M. p. 341.

‡ Ac. Par. 1734. M. p. 489.

§ Beccaria del elettriciſmo naturale et artificiale, p. 179.

THE experiments were made by threads of various kinds, and other ſubſtances, hanging down from ſilk lines, and generally made to repel one another by the approach of an excited tube. The reſult of them all he compriſed in the three following propoſitions. 1ſt. That bodies made electrical, by communication with an excited electric, are in a ſtate of repulſion with reſpect to ſuch excited bodies. 2dly. That two, or more bodies, made electrical by communicating with an excited electric, are in a ſtate of repulſion with reſpect to one another. 3dly. Excited electrics do themſelves repel one another *.

ONE of his experiments, to prove the ſecond of theſe propoſitions, deſerves to be mentioned for its curioſity. He tied a number of ſilk threads together, by a knot at each extremity; when, upon electrifying them, the threads repelled one another, and the whole bundle was ſwelled out into a beautiful ſpherical figure; ſo that he could with pleaſure, he ſays, obſerve the knot at the bottom riſing upwards, as the electricity and mutual repulſion of the threads increaſed; and he could not help imagining his bundle of ſilks to reſemble a bundle of muſcular fibres.

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* Phil. Tranſ. abridged, Vol. viii. p. 411.

By way of corollary to the ſame propoſition, he obſerves, that it ſuggeſts, more plainly than any other known experiment, a reaſon for the diſſolution of bodies in menſtrua; viz. that the particles of the ſolvend, having imbibed particles of the menſtrua, ſo as to be ſaturated with them, the ſaturated particles become repulſive of one another, ſeparate, and fly to pieces *.

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* Philoſophical Tranſactions abridged, Vol. viii. p. 410.

PERIOD V.

THE CONTINUATION AND CONCLUSION OF MR. GREY's EXPERIMENTS.

MR. GREY, upon reſuming his experiments, expreſſes great ſatisfaction, that his former obſervations had been confirmed by ſo judicious a philoſopher as Mr. Du Fay; who, he acknowledges, had made ſeveral new ones of his own, particularly that important *luciferous one*, as he calls it, recited above; which, put him upon making the experiments which follow, and which were made in the months of July and Auguſt 1734*.

As Mr. Du Fay had ſaid, that the ſnappings and the ſparks, he had mentioned, were ſtrongly excited by a piece of metal, preſented to the perſon ſuſpended on ſilk lines; Mr. Grey concluded, that if the perſon and metal ſhould change places, the effect would be the ſame. He, accordingly, ſuſpended ſeveral pieces of metal on ſilk lines, beginning with the common utenſils, which were at hand, as the poker, tongs, fire ſhovel, &c. and found, that, when they were electrified, they gave ſparks, in the ſame manner as the human body had done in like circumſtances.

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* Phil. Tranſ. abridged, Vol. viii. p. 397.

This was the origin of *metallic conductors*, which are in uſe to this day *.

MR. GREY did not, at the time above-mentioned, think of making his experiments in the dark, in order to ſee the light proceeding from the iron; not imagining, that electricity, communicated to metals would have produced ſuch ſurpriſing phenomena, as, he ſays, he afterwards found it to do.

CONTINUING his experiments at Mr. Wheeler's, they found, that the fleſh of dead animals exhibited, very nearly, the ſame appearances as that of living animals, contrary to the aſſertion of Mr. Du Fay.

BUT what moſt ſurpriſed Mr. Grey, and the gentlemen then preſent, in the experiments he made upon that occaſion, was the phenomenon above referred to, and what he now calls *a cone*, or *pencil of electric light*; ſuch as is commonly ſeen to iſſue from an electrified point. As this was the firſt time that this phenomenon, which is now ſo common, was diſtinctly ſeen, I ſhall relate the experiment, of which it was the reſult, at large.

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* Phil. Tranſ. abridged, Vol. viii. p. 398.

In order the more conveniently to communicate electricity to the iron bar, Mr. Du Fay (who adopted the contrivance from Mr. Grey) faſtened to the end of it a bundle of *linen threads*, to which he applied his excited tube. He was led to prefer the thread for this purpoſe, in conſequence of having found that, of the flexible ſubſtances, *wool*, *ſilk*, *cotton*, or *linen*, the laſt was moſt attracted by an excited electric. He ſuſpended them all to the ſame bar, and brought an excited glaſs tube to an equal diſtance from them all at the ſame time, and obſerved that they were attracted in the order in which I have mentioned them, *wool* the leaſt, and *linen* the moſt. Ac. Par. 1737. M. p. 137. Theſe linen threads of Mr. Du Fay kept their ground in a much improved ſtate of the electrical apparatus, when globes were ſubſtituted in the place of tubes, but now *ſmall wires* are univerſally uſed in preference to them.——Mr. Du Fay alſo, in order to determine what *metal* was the moſt proper for this purpoſe, procured equal cylinders of gold, ſilver, copper, braſs, lead, iron, and tin, and having placed them ſo as to conſtitute one cylinder, he drew ſparks from each of them in their turn, when the whole was electrified; but he could not, in any reſpect, perceive the leaſt difference in them. Ac. Par. 1737. M. p. 132.

MR. GREY, and his friends, provided themſelves with an iron rod four feet long, and half an inch in diameter, pointed at each end, but not ſharp. Suſpending this iron rod upon ſilk lines in the night; and applying the excited tube to one end of it, they perceived not only a light upon that end, but another iſſuing from the oppoſite end, at the ſame time. This light extended itſelf, in the form of a cone, whoſe vertex was at the end of the rod: and Mr. Grey ſays, that he and his company could plainly ſee, that it conſiſted of ſeparate threads, or rays of light, diverging from the point of the rod, the exterior rays being incurvated. This light appeared at every ſtroke they gave the tube.

THEY likewiſe obſerved, that this light was always attended with a ſmall hiſſing noiſe, which, they imagined, began at the end next the tube, increaſing in loudneſs till it came to the oppoſite end. He ſays, however, that this noiſe could not be heard, but by perſons who ſtood near the rod, and attended to it*.

MR. GREY, repeating thoſe experiments in the September following, after his return to London, obſerved an appearance, which he ſays, ſurpriſed him very much. After the tube had been applied to the iron rod, as before, when the light, which had been ſeen at both ends, had diſappeared; it was viſible again upon bringing his hand near the end of the rod; and, upon repeating this motion of his hand, the ſame phenomenon appeared for five or ſix times ſucceſſively; only the rays were, at each time, ſhorter than the other. He alſo obſerved, that theſe lights, which were emitted by the tube upon the approach of his hand, were, like the others, attended with a hiſſing noiſe.

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* Phil. Tranſ. abridged, Vol. viii. p. 398.

He took notice, that the light which appeared on the end next the tube, when it was held oblique to the axis of the rod, had its rays bending towards it; and that, all the time he was rubbing the tube, thoſe flaſhes of light appeared upon every motion of his hand up or down the tube, but that the largeſt flaſhes were produced by the motion of his hand downwards *.

When he uſed two or three rods, laying them either in a right line, or ſo as to make any angle with each other, and applied the tube to any one of their ends; he obſerved, that the farther end of the fartheſt rod exhibited the ſame phenomena as one ſingle rod †.

Using a rod pointed only at one end, he obſerved, that the other end gave but one ſingle ſnap, but that it was much louder than the greateſt of thoſe which were given by the point of the rod; alſo that the pain, reſembling pricking or burning, was more ſtrongly felt, and that the light was brighter and more contracted.

Connecting a pewter plate with the iron rod, and filling the plate with water, he obſerved the ſame light, the ſame puſhing of the finger, as he calls it, and the ſame ſnapping, as when the experiment was made with the empty plate. And when the experiment was made with water, in the day-light, it appeared to riſe in a little hill, under the finger which was preſented to it; and, after the ſnapping noiſe, fell down again, putting the water into a waving motion near the place where it had riſen.

These effects were the ſame with thoſe which he had before obſerved to proceed from the immediate action of the tube, but by theſe experiments, he ſays, he found (what, no doubt, appeared a real advance in the ſcience to him) that an actual flame of fire,

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* Phil. Tranſ. abridged, Vol. viii. p. 399.

† Ib. p. 400.

fire, together with an exploſion, and ebullition of cold water, might be produced by communicative electricity. What he adds is ſo remarkable, that I ſhall repeat his own words. "And although theſe effects are at preſent but *in minimis*, it is probable, in time, there may be found out a way to collect a greater quantity of the electric fire, and conſequently to increaſe the force of that power; which, by ſeveral of theſe experiments, *ſi licet magnis componere parva*; ſeems to be of the ſame nature with that of thunder and lightning *."

How exactly has this prophecy been fulfilled in the diſcoveries of the Leyden electricians, and Dr. Franklin; the former having diſcovered the amazing accumulation of the electric power, in what is called the Leyden phial; and the latter having proved the matter of lightning to be the very ſame with that of electricity; though Mr. Grey might poſſibly mention thunder and lightning only by way of common compariſon.

ON February the 18th, 1735, Mr. Grey, repeating the experiments of the iron rods with wooden ones, found all the effects to be ſimilar, but much weaker; as it is now well known muſt have been the caſe; wood being ſo imperfect a conductor, and only in proportion to the moiſture it contains.

AT the ſame time, he relates, that the repeating the electrification of water, he found, that the phenomena before mentioned were produced, not only by holding the tube near the water, but when it was removed, and the finger afterwards brought near it †.

MAY the 6th of the ſame year, he again ſuſpended a boy on ſilk, and found that this boy was able to communicate the electric fire, firſt to one, and then to ſeveral perſons ſtanding upon electric bodies.

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[*] Phil. Tranſ. abridged, Vol. viii. p. 401.

[†] Ibid. p. 402.

MR. GREY ſeems ſtill to have imagined, that electricity depended, in ſome meaſure, upon colour. The boy ſuſpended on blue lines, he ſays, retained his power of attraction fifty minutes; on ſcarlet lines, twenty-five minutes; and on orange coloured lines, twenty-one minutes. By theſe experiments, he ſays, we ſee the efficacy of electricity on bodies ſuſpended upon lines of the ſame ſubſtance, but of different colour [*].

BUT the greateſt deception which this ingenious gentleman ſeems to have lain under, was occaſioned by the experiments which he made with balls of iron, to obſerve the revolution of light bodies about them. The paragraph relating to theſe experiments, being the laſt which Mr. Grey wrote, I ſhall give it at length as a curioſity.

"I HAVE lately made," ſays he, "ſeveral new experiments
" upon the projectile and pendulous motion of ſmall bodies by
" electricity; by which ſmall bodies may be made to move
" about large ones, either in circles, or in ellipſes; and thoſe
" either concentric, or eccentric to the center of the larger body,
" about which they move, ſo as to make many revolutions about
" them. And this motion will conſtantly be the ſame way that
" the planets move about the ſun, viz. from the right hand to
" the left, or from weſt to eaſt. But theſe little planets, if I
" may ſo call them, move much faſter in their *apogeon*, than in
" the *perigeon* parts of their orbits; which is directly contrary to
" the motion of the planets about the ſun [†]."

THESE experiments Mr. Grey had thought of but a very little while before his laſt illneſs, and had not time to complete them; but the progreſs he had made in them he revealed, on the day before his death, to Dr. Mortimer, then ſecretary to the Royal Society. He ſaid they ſtruck him with new ſurprize every time he repeated them, and hoped that, if God would ſpare his life a little longer, he ſhould, from what theſe phenomena pointed out, bring his electrical experiments to the greateſt perfection. He did not doubt but, in a ſhort time, he ſhould be able to aſtoniſh the world with a new ſort of planetarium, never before thought of; and that, from theſe experiments, might be eſtabliſhed a certain theory, to account for the motions of the grand planetarium of the univerſe. Theſe experiments, fallacious as they are, deſerve to be briefly recited, together with thoſe which were made in conſequence of them after Mr. Grey's death. I ſhall relate them in Mr. Grey's own words, as they were delivered to Dr. Mortimer, on his death-bed.

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[*] Phil. Tranſ. abridged, Vol. viii. p. 403.

[†] Ibid. p. 404.

PLACE a ſmall iron globe, ſaid he, of an inch, or an inch and a half in diameter, on the middle of a circular cake of roſin, ſeven or eight inches in diameter, gently excited; and then a light body ſuſpended by a very fine thread, five or ſix inches long, held in the hand over the center of the table, will, of itſelf, begin to move in a circle round the iron globe, and conſtantly from weſt to eaſt. If the globe be placed at any diſtance from the center of the circular cake, it will deſcribe an ellipſe, which will have the ſame eccentricity, as the diſtance of the globe from the center of the cake.

IF the cake of roſin be of an elliptical form, and the iron globe be placed in the center of it, the light body will deſcribe an elliptical orbit, of the ſame eccentricity with the form of the cake.

IF the iron globe be placed in, or near, one of the foci of the elliptical cake, the light body will move much ſwifter in the apogee, than in the perigee of its orbit.

IF the iron globe be fixed on a pedeſtal, an inch from the table, and a glaſs hoop, or a portion of a hollow glaſs cylinder, excited, be placed round it; the light body will move as in the circumſtances mentioned above, and with the ſame varieties.

He ſaid, moreover, that the light body would make the ſame revolutions, only ſmaller, round the iron globe placed on the bare table, without any electrical body to ſupport it; but he acknowledged he had not found the experiment ſucceed, if the thread was ſupported by any thing but a human hand; though, he fancied, it would have ſucceeded, if it had been ſupported by any animal ſubſtance, living or dead *.

Mr. Grey went on to recite to Dr. Mortimer other experiments ſtill more fallacious; which, out of regard to his memory, I ſhall forbear to quote. Let the chimeras of this great electrician teach his followers, in the ſame, and ſtill but newly opened field of philoſophy, a proper degree of caution in their reaſonings from induction. Let not the example, however, diſcourage any perſon from trying what may appear improbable; but let it induce a man to delay the publication of his diſcoveries, till they have been perfectly aſcertained, and performed in the preſence of others. In experiments of great delicacy, a ſtrong imagination will have great influence even upon the external ſenſes; of which we ſhall have frequent inſtances in the courſe of this hiſtory.

Dr. Mortimer himſelf ſeems to have been deceived by theſe experiments of Mr. Grey. He ſays, that, in trying them after his death, he found, that the light body would make revolutions round bodies of various ſhapes and ſubſtances, as well as round the iron globe; and that he had actually tried the experiment, with a globe of black marble, a ſilver ſtandiſh, a ſmall chip box, and a large cork †.

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* Phil. Tranſ. abridged, Vol. viii. p. 404, 405.

† Ibid. p. 405.

THESE experiments of Mr. Grey were tried by Mr. Wheeler, and other gentlemen, at the Royal Society's houſe, and with a great variety of circumſtances; but no concluſion could be drawn from what they at that time obſerved. Mr. Wheeler himſelf took a great deal of pains to verify them, with various ſucceſs; and at laſt he gave it as his opinion, that a deſire to produce the motion from weſt to eaſt was the ſecret cauſe, that determined the pendulous body to move in that direction, by means of ſome impreſſion from Mr. Grey's hand, as well as his own; though he was, at the ſame time, perſuaded, that he was not ſenſible of giving any motion to his hand himſelf*.

MR. DU FAY, in the Memoirs of the Academy of Sciences for the year 1737, acknowledges that theſe experiments of Mr. Grey and Dr. Mortimer did not ſucceed with him. But ſays, they were the only ones of Mr. Grey that had not; and, with a temper becoming a philoſopher, adds, that he doth not, therefore, ſay, that they never had ſucceeded; but ſeems rather willing to attribute the failure with himſelf, to his omitting ſome circumſtance, not mentioned by thoſe gentlemen, though, unknown to them, it might be of principal conſequence to the experiment †. He was afterwards informed of the reſult of the laſt experiment in England on this ſubject, agreeing with his own ‡.

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* Phil. Tranſ. abridged, Vol. viii. p. 418.

† Dantzick Memoirs, Vol. i. p. 226.

‡ Ac. Par. 1737. M. p. 436.

PERIOD VI.

The experiments of Dr. DESAGULIERS.

WE are now come to the labours of that indefatigable experimental philoſopher Dr. Desaguliers, in this new field of ſcience. The reaſon which he gives why he had avoided entertaining the Royal Society upon this ſubject before, and why he had not purſued it ſo far as he might have done; conſidering, as he ſays, that he could excite as ſtrong an electricity in glaſs, by rubbing with his hand, as any body could, is worth mentioning for its curioſity, and for the light that it throws upon the temper and manner of Mr. Grey. He ſays, that he was unwilling to interfere with the late Mr. Stephen Grey, who had wholly turned his thoughts to electricity; but was of a temper to give it entirely over, if he imagined that any thing was done in oppoſition to him *.

Dr. Desaguliers begins with obſerving very ſenſibly (and the obſervation is ſtill true) that the phenomena of electricity are ſo odd, that, though we have a great many experiments upon that ſubject, we have not yet been able, from their compariſon, to ſettle ſuch a theory as will lead us to the cauſe of that property in bodies, or even to judge of all its effects, or find

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* Phil. Tranſ. abridged, Vol. viii. p. 419.

out what uſeful influence electricity has in nature; though certainly, from what we have ſeen of it, we may conjecture that it muſt be of great uſe, becauſe it is ſo extenſive.

His firſt experiments, of which an account is given in the Philoſophical Tranſactions, dated July, 1739, were made with a hempen ſtring, extended upon cat-gut. To the end of the hempen ſtring, he ſuſpended various ſubſtances; and ſays, that all thoſe which he tried, amongſt which were ſeveral *electrics per ſe*, as ſulphur, glaſs, &c. without exception, received electricity *.

He changed one of the cat-gut ſtrings, on which his hempen line of communication was extended, and put various other ſubſtances in its place, to try what bodies would tranſmit electricity to the ſuſpended body, and what would not; and from the reſult of his experiments, partly concluded, that bodies in which electricity could not be excited intercepted the electric effluvia; and that thoſe in which electricity could be excited, did not intercept it, but permitted it to go on to the extremity of the hempen ſtring. But ſtill he had no juſt idea, that, except metals, it was the moiſture in the bodies he tried which intercepted the electric effluvia; and his ideas of the manner in which they were intercepted were very imperfect.

To Dr. Deſaguliers we are indebted for ſome *technical terms* which have been extremely uſeful to all electricians to this day, and which will probably remain in uſe as long as the ſubject is ſtudied. He firſt applied the term *conductor* to that body to which the excited tube conveys its electricity; which term has ſince been extended to all bodies that are capable of receiving that virtue. And he calls thoſe bodies in which electricity may be excited, by heating or rubbing, *electrics per ſe*.

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* Phil. Tranſ. abridged, Vol. viii. p. 420.

IN the writings of this author we find many *axioms* relating to electrical experiments, ſome of which are expreſſed in a more clear, and diſtinct manner than they had been before; but the real improvements which he made, were very few and immaterial.

ON ſeveral occaſions, and particularly in a paper delivered to the Royal Society, in the month of January 1741, he lays down, among others, the following general rules, which ſeem to be more accurate than any which had been delivered before upon the ſubject *.

" AN electric per ſe will not receive electricity from another " electric per ſe, in which it has been excited, ſo as to run " along its whole length: but will only receive it a little way, " being, as it were, ſaturated with it.

" AN electric per ſe will not loſe all its electricity at once, " but only the electricity of thoſe parts near which a non-" electric has been brought. It, conſequently, loſes its electricity " the ſooner, the more of thoſe bodies are near it. Thus, in moiſt " weather, the excited tube holds its virtue but a little while, be-" cauſe it acts upon the moiſt vapours which float in the air. " And if the excited tube be applied to leaf gold laid upon a " ſtand, it will act upon it much longer, and more ſtrongly, " than if the ſame quantity of leaf gold be laid upon a table, " which has more non-electric ſurface than the ſtand †." This, however, ſeems not to be the whole reaſon; for if the leaf gold were laid upon a broad ſurface of glaſs, it would not be acted upon ſo powerfully, as if it were placed upon a narrow ſtand of any kind of matter.

" A NON-ELECTRIC, when it has received electricity, loſes it " all at once, upon the approach of another non-electric." This however could only be the caſe when the approaching electric was not inſulated, but had a communication with the earth. It muſt alſo be brought into contact with the electrified body.

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* Phil. Tranſ. abridged, Vol. viii. p. 430.

† Ibid. p. 427.

"ANIMAL ſubſtances are non-electrics by reaſon of the fluids they contain [*].

"EXCITED electricity exerts itſelf in a ſphere round the "electric per ſe, or rather in a cylinder, if the body be cylin-"drical [†]."

FEW of the many experiments which were made by Dr. Deſaguliers (accounts of which were publiſhed in the Philoſophical Tranſactions) had, as I obſerved before, any thing new in them. Thoſe which were the moſt ſo are the following.

ENDEAVOURING to communicate electricity to a burning tallow candle, he obſerved, that the candle attracted the thread of trial, but not within two or three inches of the flame; but that, as ſoon as the candle was blown out, the thread was attracted by every part of it, and even by the wick, when the fire was quite extinguiſhed. He electrified a wax candle in the ſame manner, and the experiment ſucceeded as well, only the electricity came not ſo near the flame in the wax, as in the tallow candle.

HE ſays, that only warming a glaſs receiver, without any rubbing, would cauſe the threads of a down feather, tied to an upright ſkewer to extend themſelves, as ſoon as it was put over the feather; and that ſometimes roſin and wax would exert their electricity by being only expoſed to the open air.

HE obſerved that if a hollow glaſs tube, ſupporting the line of communication, were moiſtened by blowing through it, it would intercept the electricity.

HE ſays, that when an excited tube has repelled a feather, it will attract it again, after being ſuddenly dipped into water, but in fair weather it will not attract it unleſs it hath been dipped pretty deep into the water, a foot of its length at leaſt; whereas, in moiſt weather, an inch or two will ſuffice *.

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[*] Phil. Tranſ. abridged, Vol. viii. p. 429.

[†] Ibid. p. 431.

HE ſhewed the attraction of water by an excited tube, in a better manner than it had been ſhewn before, viz. by bringing the tube to a ſtream iſſuing from a condenſing fountain: which, thereupon, was evidently bent towards it.

DR. DESAGULIERS ſeems to have been the firſt who expreſsly ſaid, that pure *air* might be ranked amongſt electrics per ſe, and that cold air in froſty weather, when vapours riſe leaſt of all, is preferable, for electrical purpoſes, to warm air in ſummer, when the heat raiſes the vapours †. He alſo ſuppoſed that the electricity of the air was of the vitreous kind; and he accounted for the electricity appearing on the inſide only of an exhauſted glaſs veſſel, by its going where it met with the leaſt reſiſtance from ſo electrical a body as the air ‡.

HE endeavoured to account for the fixing of air by the ſteams of ſulphur, according to the experiment of Dr. Hales; by ſuppoſing that the particles of ſulphur, and thoſe of air, being poſſeſſed of different kinds of electricity, attracted one another, whereby their repulſive power was deſtroyed. He alſo propoſed the following conjecture concerning the riſe of vapour. The air at the ſurface of water being electrical, particles of water, he thought, jumped to it, then, becoming themſelves electrical, they repelled both the air and one another, and conſequently aſcended into the higher regions of the atmoſphere §.

THE laſt paper of Dr. Deſaguliers in the Philoſophical Tranſactions, upon the ſubject of electricity, is dated June 24th, 1742, in which year he publiſhed a diſſertation on electricity,

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* Phil. Tranſ. abridged, Vol. viii. p. 429.

† Ib. p. 437.

‡ Ibid. p. 438.

§ Ibid. 437.

city, by which he gained the prize of the academy at Bourdeaux. This prize was a medal of the value of 300 livres, propoſed, at the requeſt of Monſieur Harpez de la Force, for the beſt eſſay on electricity, and ſhews how much this ſubject engaged the attention of philoſophers at that time*. The diſſertation is well drawn up, and compriſes all that was known of the ſubject till that period.

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* Dantzick Memoirs, Vol. i. p. 261.

PERIOD VII.

Experiments of the GERMANS, and of Dr. WATSON, before the discovery of the Leyden phial in the year 1746.

ABOUT the time that Dr. Deſaguliers had concluded his experiments in England, viz. 1742, ſeveral ingenious Germans began to apply themſelves to the ſame ſtudies with great aſſiduity, and their labours were crowned with conſiderable ſucceſs.

To the Germans we are indebted for many capital improvements in our electrical apparatus within this period, without which, the buſineſs would have gone on very ſlowly and heavily; but, by the help of their contrivances, we ſhall ſee that aſtoniſhing effects were ſoon produced.

Mr. Boze, a profeſſor of philoſophy at Wittemburgh, ſubſtituted the *globe* for the tube, which had been uſed ever ſince the time of Haukſbee[*]. He likewiſe added a *prime conductor*, which conſiſted of a tube of iron or tin, at firſt ſupported by a man ſtanding upon cakes of roſin, and afterwards ſuſpended on ſilk horizontally before the globe [*].

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[*] According to other accounts, Chriſtian Auguſtus Hauſen, profeſſor of mathematics at Leipſic, was the firſt who revived the uſe of Haukſbee's glaſs globe, and Mr. Boze, who was excited to make experiments in electricity by the example of Mr. Hauſen, borrowed this capital improvement from him. Dantzick Memoirs, Vol. i. p. 278, 279.

To prevent the tube from doing any harm to the globe, he put a bundle of thread into the end which was next to it, and which was left open for that purpoſe. This expedient, beſides occaſioning various pleaſant phenomena, was obſerved to make the force of the conductor much ſtronger [†].

The uſe of the globe was immediately adopted in the univerſity of Leipſic, where Mr. Winckler, the profeſſor of languages, ſubſtituted a *cuſhion* inſtead of the hand, which had before been employed to excite the globe. But the beſt rubber for the globe, as well as the tube, was, long after this, ſtill thought, by all electricians, to be the human hand, dry, and free from moiſture [‡].

Mr. P. Gordon, a Scotch Benedictine monk, and profeſſor of Philoſophy at Erford, was the firſt who uſed a *cylinder* inſtead of a globe. His cylinders were eight inches long, and four inches in diameter. They were made to turn with a bow, and the whole inſtrument was portable. Inſtead of a cake of roſin, he inſulated by means of a frame, furniſhed with net work of ſilk [§].

The apparatus, likewiſe, of many of the German electricians was very *various*, and expenſive. Mr. Winckler, in a paper read at the Royal Society, March 21ſt, 1745 [||], deſcribes a machine for rubbing tubes, and another for rubbing globes, and compares the effects of them both. He obſerves, that the ſparks which are produced from glaſs veſſels drawn to and fro were larger, and more vehemently pungent, provided that thoſe veſſels were of the ſame magnitude with the globes; but that the flux of efluvia was not ſo conſtant as from the globes. Mr. Winckler alſo invented a machine, which he deſcribes at large in his works, by means of which he could give his globe ſix hundred and eighty turns in a minute *. This gentleman likewiſe contrived to rub glaſs, and china veſſels, in the inſide; and he ſays they acted as ſtrongly on bodies placed on the outſide of them, as when they were rubbed on the outſide †.

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[*] Hiſtoire de l'electricité, p. 27.

[†] Phil. Tranſ. abridged, Vol. x. p. 271.

[‡] Ibid. 272.

[§] Hiſtoire, p. 31.

[||] Phil. Tranſ. abridged, Vol. x. p. 273.

The German electricians generally uſed more globes than one at a time, and imagined they found the effects proportionable, though this fact was called in queſtion by Dr. Watſon, and others; and Mr. Nollet preferred globes made blue with zaffre ‡, which were carefully tried, and rejected by Dr. Watſon afterwards §.

Such a prodigious power of electricity could they excite from theſe globes, whirled by a large wheel, and rubbed with woollen cloth, or a dry hand (for we find both theſe methods were in uſe among them about this time) that, if we may credit their own accounts, the blood could be drawn from the finger by an electric ſpark; the ſkin would burſt, and a wound appear, as if made by a cauſtic. They ſay, that if ſeveral globes or tubes were uſed, the motion of the heart and arteries of the electrified perſon would be very ſenſibly increaſed; and that, if a vien were opened under the operation, the blood, iſſuing from it, would appear like lucid phoſphorus, and run out faſter than when the man was not electrified. Analogous to this laſt experiment, Mr. Gordon obſerved, that water, running from an artificial fountain electrified, was ſcattered in luminous drops, that a larger quantity of water was thrown out in a given time than when the fountain was not electrified ‖; and that electrified

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* Hiſtoire, p. 32.

† Dantzick Memoirs, Vol. i. p. 400.

‡ In ſubſequent trials the Abbé himſelf found no advantage in thoſe blue globes. Ac. Par. 1745. M. p. 162.

§ Phil. Tranſ. abridged, Vol. x. p. 418.

‖ Ibid. p. 277.

water evaporated faſter than water not electrified, when expoſed in ſimilar glaſs veſſels[*]. Part of this account we know might be true, but ſome part muſt have been exaggerated. It is certain that Mr. Gordon increaſed the electric ſparks to ſuch a degree, that they were felt from a man's head to his foot, ſo that a perſon could hardly take them without falling down with giddineſs[†], and ſmall birds were killed by them[‡]. This he effected by conveying electricity, with iron wires, to the diſtance of 200 ells from the place of excitation. He alſo found that the ſparks were ſtronger when the wire was thick than when it was ſmall[§].

MR. WAITZ made his glaſs tubes act ſtronger by rubbing them with a waxed cloth and a little oil. He alſo found, that glaſs made with very little pot-aſh, acted much better than that in the compoſition of which much of it was uſed; but it required longer time, and more heat to vitrify it. He got ſome glaſs made on purpoſe to aſcertain the fact[‖].

BY various experiments of attraction and repulſion, which Mr. Waitz made in rubbing a dog (which he had made thoroughly dry for that purpoſe) he proved that the flaſhes of light, which ſometimes appear when animals are ſtroked, are electrical. This had been ſuppoſed, but was not accurately aſcertained before[¶]. It was this gentleman who gained the prize of 50 ducats, propoſed, in the year 1744, by the Academy of Sciences at Berlin, for the beſt diſſertation on the ſubject of electricity. It was publiſhed along with three others, which were offered at the ſame time, and thought worthy of that honour[**].

THE thing that ſtrikes us moſt in their experiments, performed by theſe machines, is their ſetting fire to inflammable ſubſtances. This they were, probably, led to attempt, from obſerving the vivid appearance of electric light, the burning pain that was felt by a ſmart ſtroke from the conductor, and the many analogies the electric fluid evidently bore to phoſphorus and common fire.

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[*] Dantzick Memoirs, Vol. ii. p. 357.

[†] Ibid. p. 358.

[‡] Nollet's Recherches, p. 172.

[§] Dantzick Memoirs, Vol. ii. p. 359.

[‖] Ibid. p. 381.

[¶] Ibid. p. 385.

[**] Ibid. p. 380.

THE firſt perſon who ſucceeded in this attempt was Dr. Ludolf of Berlin, towards the beginning the year 1744; who kindled, with ſparks excited by the friction of a glaſs tube, the etherial ſpirit of Frobenius. This he did at the opening of the Royal Academy, and in the preſence of ſome hundreds of perſons. He performed the experiment by electric ſparks proceeding from an iron conductor. Mr. Winckler did the ſame in the May following, by a ſpark from his own finger; and kindled, not only the highly rectified ſpirit above-mentioned, but French brandy, corn ſpirits, and other ſpirits ſtill weaker, by previouſly heating them. He alſo ſays, that oil, pitch, and ſealing-wax might be lighted by electric ſparks, provided thoſe ſubſtances were firſt heated to a degree next to kindling *. To theſe it muſt be added, that Mr. Gralath fired the ſmoke of a candle juſt blown out, and lighted it again †; and that Mr. Boze fired gunpowder, melting it in a ſpoon, and firſt the vapour that roſe from it.

THE German electricians, likewiſe, conſtructed a machine, by which they could give friction to a glaſs cylinder in vacuo. By theſe means they contrived to electrify a wire which terminated in the open air, and there ſhewed a conſiderable electric power. They alſo electrified that end which was in the open air, and made the other end which was in vacuo exert its electricity ‡.

THE ſame Germans alſo mention an experiment, which, if purſued, would have led them to diſcover, that the friction of the glaſs globe did not produce, but only collect the electric matter.

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* Phil. Tranſ. abridged, Vol. x. p. 271.

† Dantzick Memoirs, Vol. ii. p. 438.

‡ Phil. Tranſ. abridged, Vol. x. p. 275.

matter. But that was a diſcovery reſerved, as we ſhall find, for Dr. Watſon. It ſeems that both Mr. Boze and Mr. Allamand had ſuſpended the machine, and the man who worked it, upon ſilk; and obſerved, that, not only the conductor, but alſo the man and the machine gave ſigns of electricity; though they did not attend accurately to all the circumſtances of that curious fact, which did not at all anſwer their expectations. For, imagining that part of the electric power was continually going off to the ground by the machine, they ſuppoſed that the effect of inſulating it, would have been a ſtronger electricity *.

IN this period it was that Ludolf the younger demonſtrated, that the luminous barometer is made perfectly electrical by the motion of the quickſilver; firſt attracting, and then repelling bits of paper, &c. ſuſpended by the ſide of the tube, when it was encloſed in another out of which the air was extracted †. Before this experiment, thoſe effects had been aſcribed to the air ‡. Profeſſor Hamberger and Mr. Waitz had diſcovered that the motion of quickſilver in a glaſs veſſel, out of which the air was extracted, had the power of moving light bodies; and Mr. Allamand likewiſe found, that it made no difference whether the veſſel had air in it or not §.

ABOUT the ſame time alſo, Mr. Boze took a great deal of pains to determine, whether the weight of bodies would be affected by electricity, but he could not find that it was.

THE electrical *ſtar*, made by turning ſwiftly round an electrified piece of tin, cut with points equidiſtant from the center; and alſo the *electrical bells*, which will be deſcribed hereafter among the ſurpriſing and diverting experiments performed by the help of electricity, were of German invention ||. The ſtar was con-

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* Wilſon's Eſſay, preface, p. 14. Watſon's Sequel, p. 34.

† Dantzick Memoirs, Vol. iii. p. 495.

‡ Hiſtoire, p. 89.

§ Dantzick Memoirs, Vol. ii. p. 426.

|| Nollet's Recherches, p. 187.

trived by Mr. Gordon, and by turning the points a little obliquely, he was ſurpriſed to find it began to move of itſelf *. Laſtly, to theſe it may be added, that Mr. Winckler contrived a wheel to move by electricity; that Mr. Boze conveyed electricity from one man to another by a jet of water, when they were both placed upon cakes of roſin, at the diſtance of ſix paces; and that Mr. Gordon even fired ſpirits by a jet of water †.

MR. GOTTFRIED HEINRICK GRUMMERT, of Biala in Poland, made a curious experiment upon electric light, which, as we ſhall ſee, was afterwards made and purſued to great advantage by Dr. Watſon and Mr. Canton. In order to obſerve whether an exhauſted tube would give light when it was electrified, as well as when it was excited, he preſented one, eight inches long, and a third of an inch wide, to the electrified conductor, and was ſurpriſed to find the light dart very vividly the whole length of the tube. He alſo obſerved, that, ſome time after the tube had been preſented to the conductor, and expoſed to nothing but the air, it gave light again, without being brought to any electrified body. This light *in vacuo* Mr. Grummert propoſed to make uſe of in mines, and places where common fires, and other lights cannot be had, and for this purpoſe he mentions ſeveral methods of increaſing this light ‡.

I SHALL conclude this account of the diſcoveries of the German Philoſophers in this period with a very curious one of profeſſor Kruger, concerning the change made in the colour of bodies by the electric effluvia. In order to try, whether there was any thing of ſulphur in theſe effluvia, he expoſed the red leaves of wild poppies to the electric ſpark, and found that they were preſently changed to white. He was not able to produce any change in yellow colours, nor in blue immediately;

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* Dantzick Memoirs, Vol. ii. p. 317, 358.

† Phil. Tranſ. abridged, Vol. x. p. 276.

‡ Dantzick Memoirs, Vol. i. p. 417.

but found that when they had lain a day or two, after being expoſed to this operation they became white. In theſe experiments the leaves were faſtened with white wax to plates of tin *.

SUCH a general attention was excited to electricity by theſe curious diſcoveries, that, in the year 1745, electrical experiments were exhibited, in Germany and Holland, for money, as a ſhow; and public advertiſements appeared in the news-papers for that purpoſe †.

THE firing of the effluvia of bodies, which was firſt done in Germany, was ſoon after repeated in England, and among others by Dr. Miles; who, as appears by a paper of his, read at the Royal Society, March the 7th, 1745, kindled phoſphorus by the application of the excited tube itſelf to it without the intervention of any conductor ‡.

THIS gentleman's tube happening to be in excellent order upon this occaſion, he obſerved, and was perhaps the firſt who obſerved, *pencils of rays*, which he calls *corruſcations*, darting from the tube, without the aid of any conductor approaching it. Of theſe corruſcations he gave a drawing, which anſwers pretty exactly to the appearance of ſuch pencils as are now very common, particularly ſince Mr. Canton has taught us the uſe of the amalgama, by which a tube may be excited much more ſtrongly than it could have been before §.

BUT the moſt diſtinguiſhed name in this period of the hiſtory of electricity, is that of Dr. WATSON. He was one of the firſt among the Engliſh who took up, and improved upon, the diſcoveries made by the Germans; and to his ingenuity, and intenſe application, we owe many curious improvements and diſcoveries in electricity. His firſt letters to the Royal Society on this ſubject are dated between March 28th, and October 24th, 1745.

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* Dantzick Memoirs, Vol. iii. p. 393.

† Ibid. Vol. ii. p. 399.

‡ Phil. Tranſ. abridged, Vol. x. p. 272.

§ Ibid. p. 272.

Dr. Watson's attention to the ſubject of electricity ſeems firſt, or principally, to have been engaged by the accounts of the Germans having fired ſpirit of wine by it. In this experiment he ſucceeded; and, moreover, found that he was able to fire, not only the ethereal ſpirit of Frobenius, and rectified ſpirit of wine, but even common proof ſpirit. He alſo fired air made inflammable by a chemical proceſs *. He even fired both ſpirit of wine, and inflammable air, by a drop of cold water, thickened with a mucilage made with the ſeed of flea-wort, and even with ice †. He alſo fired theſe ſubſtances with a hot poker electrified, when it would not fire them in any other ſtate ‡. He fired gunpowder and diſcharged a muſket by the power of electricity, when the gunpowder had been ground with a little camphor, or a few drops of ſome inflammable chemical oil §. Laſtly, it was a diſcovery of Dr. Watſon's, that theſe ſubſtances were capable of being fired by what he calls *the repulſive power of electricity*; which was performed by the electrified perſon holding the ſpoon which contained the ſubſtance to be fired, and another perſon, not electrified, bringing his finger to it ||. Before this time, the ſubſtance to be fired had always been held by a perſon not electrified.

In his attempts to fire electrics per ſe, as turpentine, and balſam of capivi, by this repulſive power, he thought he confuted an opinion which had prevailed among many perſons, that electricity only floated on the ſurfaces of bodies, for he found that the fume of theſe ſubſtances could not be fired by a ſpark fetched from the ſpoon which contained them. This ſpark muſt therefore paſs through the electric, from the ſurface

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* Phil. Tranſ. abridged, Vol. x. p. 286.

† Ib. p. 290.

‡ Ibid. p. 288.

§ Ibid. p. 289.

|| Ibid. p. 281.

of the ſpoon below, which was in contact with the electrified conductor.

ELECTRIFYING a number of pieces of fine ſpun glaſs, and other pieces of wire, of the ſame length and thickneſs, he was agreeably amuſed by obſerving, that the threads of glaſs jumped to the electrified body, and adhered to it without any ſnapping; whereas the wires jumped up and down very faſt, giving a ſnap, and a ſmall flame, every time *.

IN a paper read at the Royal Society, February 6th, 1746, he obſerved, that electric ſparks appeared different in colour and form, according to the ſubſtances from which they proceeded; that the fire appeared much redder from rough bodies, as ruſty iron, &c. than from poliſhed bodies, though they were ever ſo ſharp, as from poliſhed ſciſſars, &c. He judged that the different appearance was owing rather to the different reflection of the electric light from the ſurface of the bodies from which it was emitted, than to any difference in the fire itſelf †.

HE alſo obſerved, that electricity ſuffered no refraction in pervading glaſs; having found, by exact obſervations, that its direction was always in right lines, even through glaſſes of different forms, included one within another, and large ſpaces left between each glaſs ‡; that if books or other non-electrics were laid upon glaſs and interpoſed between the excited electric and light bodies, the direction of the virtue was ſtill in right lines, and ſeemed inſtantly to paſs through both the books and the glaſs. In theſe experiments he conſtantly obſerved, that the electric attraction through glaſs was much more powerful when the glaſs was made warm than when it was cold §. He ſometimes found electricity to pervade, though in ſmall quantities, electrics of above four inches thick ‖.

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* Phil. Tranſ. abridged, Vol. x. p. 286. † Ibid. p. 290.

‡ Ibid. p. 291. § Ibid. p. 292. ‖ Ibid. p. 295.

HE ſays, that in electrifying ſubſtances of great extent, the attractive power was firſt obſerved at that part of it which was moſt remote from the excited electric.

HE made ſome experiments which ſhowed, that the fire of electricity was affected, neither by the preſence, nor the abſence of other fire. One of his experiments was made with a chemical mixture, thirty degrees below the freezing point of Fahrenheit's thermometer; from which, when electrified, the flaſhes were as powerful, and the ſtrokes as ſmart, as from red hot iron *.

IN a ſequel to the above experiments, read the 30th of October 1746, Dr. Watſon mentions his having lined a glaſs globe to a conſiderable thickneſs with a mixture of wax and roſin; but he found no difference between that and the other globes †.

HE alſo made various experiments with a number of globes, whirled at the ſame time, and having one common conductor; and concluded from them, that the power of electricity was increaſed by the number and ſize of the globes, to a certain degree, but by no means in proportion to their number and ſize. Yet the Doctor allows a very great increaſe, in an inference he makes from theſe very experiments. As bodies to be electrified, he ſays, will only contain a certain quantity of electricity; when that quantity is acquired, *which is ſooneſt done by a number of globes*, the ſurcharge is diſſipated as faſt as it is excited. So that, it is plain, more fire was collected by the number of globes, though the form of the conductor he made uſe of was ſuch as could not retain it. The great power of his four globes united, is manifeſt from his own account of them. For, he ſays, that when two pewter plates were held, one in the hand of an electrified perſon, and the other in the hand of one who ſtood upon the floor; the flaſhes of pure and bright flame were ſo large, and ſucceeded each other ſo faſt, that, when the room was darkened, he could diſtinctly ſee the faces of thirteen perſons who ſtood round the room *.

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* Phil. Tranſ. abridged, Vol. x. p. 293.

† Ibid. p. 295.

LASTLY, the Doctor found, that the ſmoke of original electrics was a conductor of electricity, and alſo that flame would conduct the whole of it undiminiſhed; by obſerving that two perſons, ſtanding upon electrics, could communicate the virtue to each other, with nothing interpoſed but the ſmoke in the one caſe, and flame in the other †.

IT was in this period that Mr. Du Tour diſcovered that flame would deſtroy electricity; as he informed the Abbé Nollet, in a letter dated 21ſt Auguſt 1745. The ſame was alſo diſcovered by Mr. Waitz.

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* Phil. Tranſ. abridged, Vol. x. p. 295.

† Ibid. p. 296.

PERIOD VIII.

THE HISTORY OF ELECTRICITY, FROM THE DISCOVERY OF THE LEYDEN PHIAL IN THE YEARS 1745 AND 1746, TO DR. FRANKLIN's DISCOVERIES.

SECTION I.

THE HISTORY OF THE LEYDEN PHIAL ITSELF, TILL DR. FRANKLIN's DISCOVERIES RELATING TO IT.

THE end of the year 1745, and the beginning of 1746 were famous for the moſt ſurpriſing diſcovery that has yet been made in the whole buſineſs of electricity, which was the wonderful accumulation of its power in glaſs, called at firſt the LEYDEN PHIAL; becauſe made by Mr. Cuneus a native of Leyden, as he was repeating ſome experiments which he had ſeen with Meſſrs. Muſchenbroeck, and Allamand, profeſſors in the univerſity of that city *. But the perſon who firſt made this great diſcovery, was Mr. Von Kleiſt, dean of the cathedral in Camin; who, on the 4th of November 1745, ſent an account of it to Dr. Lieberkuhn at Berlin. This account, as taken by Mr.

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* Dalibard's Hiſtoire abrigée, p. 33.

Mr. Gralath out of the regiſter of the academy at Berlin, to which it had been communicated, is as follows. " When a " nail, or a piece of thick braſs wire, &c. is put into a ſmall " apothecary's phial and electrified, remarkable effects follow: " but the phial muſt be very dry, or warm. I commonly rub " it over before-hand with a finger, on which I put ſome pounded " chalk. If a little mercury or a few drops of ſpirit of " wine, be put into it, the experiment ſucceeds the better. As " ſoon as this phial and nail are removed from the electrifying " glaſs, or the prime conductor, to which it hath been expoſed, " is taken away, it throws out a pencil of flame ſo long, " that, with this burning machine in my hand, I have taken " above ſixty ſteps, in walking about my room. When it is " electrified ſtrongly, I can take it into another room, and there " fire ſpirits of wine with it. If while it is electrifying, I put " my finger, or a piece of gold, which I hold in my hand, " to the nail, I receive a ſhock which ſtuns my arms and " ſhoulders.

" A TIN tube, or a man, placed upon electrics, is electrified " much ſtronger by this means than in the common way. When " I preſent this phial and nail to a tin tube, which I have, fifteen " feet long, nothing but experience can make a perſon believe " how ſtrongly it is electrified. I am perſuaded, he adds, that, " in this manner, Mr. Boze would not have taken a ſecond " electrical kiſs. Two thin glaſſes have been broken by the " ſhock of it. It appears to me very extraordinary, that when " this phial and nail are in contact with either conducting or " non-conducting matter, the ſtrong ſhock does not follow. " I have cemented it to wood, metal, glaſs, ſealing-wax, &c. " when I have electrified without any great effect. The human " body, therefore, muſt contribute ſomething to it. This " opinion is confirmed by my obſerving, that, unleſs I

" hold

"hold the phial in my hand, I cannot fire ſpirits of wine "with it *."

Notwithstanding Mr. Kleiſt immediately communicated an account of this famous experiment (which indeed it is evident he has but imperfectly deſcribed) to Mr. Winckler at Leipſick, Mr. Swiettiki of Dantzick, Mr. Kruger of Hall, and to the profeſſors of the academy of Lignitz, as well as to Dr. Lieberkuhn of Berlin above mentioned, they all returned him word, that the experiment did not ſucceed with them. Mr. Gralath of Dantzick, was the firſt with whom it anſwered; but this was not till after ſeveral fruitleſs trials, and receiving farther inſtructions from the inventor †.

The Abbé Nollet had information of this diſcovery, and, in conſequence of it, ſays, in a letter to Mr. Samuel Wolfe, of the Society of Dantzick, dated March 9th, 1746, that the experiment at Leyden was upon principles ſimilar to this made with a phial half full of water, and a nail dipped in it; and that this diſcovery would have been called the Dantzick experiment, if it had not happened to have got the name of that of Leyden ‡.

The views which led to this diſcovery in Holland were, as I have been informed, as follows. Profeſſor Muſchenbroeck and his friends, obſerving that electrified bodies, expoſed to the common atmoſphere, which is always replete with conducting particles of various kinds, ſoon loſt their electricity, and were capable of retaining but a ſmall quantity of it, imagined, that, were the electrified bodies terminated on all ſides by original electrics, they might be capable of receiving a ſtronger power, and retaining it a longer time. Glaſs being the moſt convenient electric for this purpoſe, and water the moſt convenient non-electric, they firſt made theſe experiments with water, in glaſs bottles: but

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* Dantzick Memoirs, Vol. i. p. 407.

† Ibid. p. 411.

‡ Ibid. p. 409.

but no conſiderable diſcovery was made, till Mr. Cuneus, happening to hold his glaſs veſſel in one hand, containing water, which had a communication with the prime conductor, by means of a wire; and, with the other hand, diſengaging it from the conductor (when he imagined the water had received as much electricity as the machine could give it) was ſurpriſed by a ſudden ſhock in his arms and breaſt, which he had not in the leaſt expected from the experiment.

Mr. Allamand and Mr. Muſchenbroeck were the firſt who repeated and publiſhed an account of this experiment in Holland, the Abbé Nollet and Monſieur Monnier in France, and Meſſrs. Gralath and Rugger in Germany *.

It is extremely curious to obſerve the deſcriptions which philoſophers, who firſt felt the electrical ſhock, give of it; eſpecially as we are ſure we can give ourſelves the ſame ſenſation, and thereby compare their deſcriptions with the reality. Terror and ſurprize certainly contributed not a little to the exaggerated accounts they gave of it; and, could we not have repeated the experiment, we ſhould have formed a very different idea of it from what it really is, even when given in greater ſtrength than thoſe who firſt felt this electrical ſhock were able to give it. It will amuſe my readers if I give them an example or two.

Mr. Muschenbroeck, who tried the experiment with a very thin glaſs bowl, ſays, in a letter to Mr. Reaumur, which he wrote ſoon after the experiment, that he felt himſelf ſtruck in his arms, ſhoulder, and breaſt, ſo that he loſt his breath, and was two days before he recovered from the effects of the blow and the terror. He adds, that he would not take a ſecond ſhock for the kingdom of France †.

The firſt time Mr. Allamand made this experiment (which was only with a common beer glaſs) he ſays, that he loſt the uſe of

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* Dantzick Memoirs, Vol. ii. p. 433.

† Hiſtoire de l'electricité, p. 30.

of his breath for ſome moments; and then felt ſo intenſe a pain all along his right arm, that he at firſt apprehended ill conſequences from it, though it ſoon after went off without any inconvenience *. But the moſt remarkable account is that of Mr. Winckler of Leipſic. He ſays, that the firſt time he tried the Leyden experiment, he found great convulſions by it in his body; and that it put his blood into great agitation; ſo that he was afraid of an ardent fever, and was obliged to uſe refrigerating medicines. He alſo felt an heavineſs in his head, as if a ſtone lay upon it. Twice, he ſays, it gave him a bleeding at his noſe, to which he was not inclined. His wife (whoſe curioſity, it ſeems, was ſtronger than her fears) received the ſhock only twice, and found herſelf ſo weak, that ſhe could hardly walk; and, a week after, upon recovering courage to receive another ſhock, ſhe bled at the noſe after taking it only once †.

We are not, however, to infer from theſe inſtances, that all the electricians were ſtruck with this panic. Few, I believe, would have joined with the cowardly profeſſor, who ſaid that he would not take a ſecond for the kingdom of France. Far different from theſe were the ſentiments of the magnanimous Mr. Boze, who with a truly philoſophical heroiſm, worthy of the renowned Empedocles, ſaid he wiſhed he might die by the electric ſhock, that the account of his death might furniſh an article for the memoirs of the French Academy of Sciences ‡. But it is not given to every electrician to die in ſo glorious a manner as the juſtly envied Richman.

It was this aſtoniſhing experiment that gave eclat to electricity. From this time it became the ſubject of general converſation. Every body was eager to ſee, and, notwithſtanding the terrible account that was reported of it, to *feel* the experiment; and in the ſame year in which it was diſcovered, numbers of perſons, in almoſt every country in Europe, got a livelihood by going about and ſhowing it.

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* Phil. Tranſ. abridged, Vol. x. p. 321. † Ibid. 327. ‡ Hiſtoire, p. 164.

While the vulgar of every age, ſex, and rank were viewing this prodigy of nature and philoſophy with wonder and amazement, we are not ſurpriſed to find all the electricians of Europe immediately employed in repeating this great experiment, and attending to the circumſtances of it. Mr. Allamand remarked, that, when he firſt tried it, he ſtood ſimply upon the floor, and not upon cakes of roſin. He ſaid, that it did not ſucceed with all kinds of glaſs; for that, though he had tried ſeveral, he had perfect ſucceſs with none but that of Bohemia, and that he had tried Engliſh glaſſes without any effect at all *. Profeſſor Muſchenbroeck at that time only obſerved, that the glaſs muſt not be all wet on the outſide.

It is no wonder that ſo few of the properties of glaſs charged with electrical fire were known at firſt, notwithſtanding the attention that was immediately given to the ſubject by all the electricians in Europe. The experiment is, to this day, juſtly viewed with aſtoniſhment by the moſt profound electricians: for, though ſome remarkable phenomena of it have been excellently accounted for by Dr. Franklin, and others, much remains to be done; and, in many reſpects, the circumſtances attending it are ſtill inexplicable. What will reſult from more attention being given to it, time only can ſhow.

To begin the farther illuſtration of this diſcovery with ſuch of the phenomena as were obſerved in Germany, where it was made. Mr. Gralath made the ſhock much ſtronger by uſing a glaſs veſſel five inches in diameter, with a narrow neck, ten inches long; by ſubſtituting an iron wire with a knob of

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* Phil. Tranſ. abridged, Vol. x. p. 321.

tin for the iron nail, and water for ſpirit of wine *. He firſt found, that the ſame ſhock could be communicated to a number of perſons, who took hold of one another's hands; if the perſon at one extremity of the line they made, touched the outſide of the phial, and he at the other touched a wire communicating with the inſide. In this manner, on the 10th of April, 1746, he gave a ſhock to twenty perſons; and he ſays, he did not doubt, but it might be given to a thouſand †. When theſe perſons were connected by pieces of metal, and did not hold one another's hands, they found the ſame ſhock, but not when they held wood, and other imperfect conductors ‡. This gentleman, alſo, gave a ſhock, by means of long wires, to a perſon ſtanding in a garden, while he himſelf, who directed the experiment, had the machine in a part of the houſe at a conſiderable diſtance §; and he was the firſt who made what we now call an *electrical battery*; for he increaſed the ſhock by charging ſeveral phials at the ſame time ‖. Laſtly he obſerved that if the phial had the leaſt crack in it, it could never give a ſhock; and alſo that when a phial was diſcharged it acquired a ſmall charge by ſtanding, without receiving any thing from the machine, ſo as to give a ſmall ſhock ¶. This is what we now call the *reſiduum* of a charge, and is properly that part of the charge that lay on the uncoated part of the phial, which doth not let go all its electricity at once; ſo that it is, afterwards, gradually diffuſed to the coating.

MR. WINCKLER found out the method of making the diſcharge of the phial without feeling the ſhock himſelf, by not bringing his own body into the direct circuit **. He alſo gave the ſhock when ſeveral ells of running water, in his garden, made part of the circuit. This he did on the 28th of July 1746, about the ſame time that Monſieur Monnier performed the ſame experiment in France *. And, laſtly, he found, that the more globes were uſed, and the larger they were, the ſtronger was the ſhock †. This muſt be the conſequence of increaſing the power of excitation.

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* Dantzick Memoirs, Vol. ii. p. 411. † Ibid. p. 439. ‡ Ibid. p. 440. § Ibid. ‖ Ibid. p. 552. ¶ Ibid. Vol. i. p. 514, 516. ** Ibid. Vol. ii. p. 459.

When Mr. Jallabert made the Leyden experiment with hot water, the phial broke by a ſpontaneous diſcharge, and a circular piece, two lines and a half in diameter, was thrown from the place of the rupture, againſt a wall, which was at the diſtance of five feet. The veſſel, he ſays, had no crack, or other injury ‡.

Dr. Watson, who gives an account of this famous experiment in the Philoſophical Tranſactions, obſerves, that it ſucceeded beſt when the phial, which contained the water, was of the thinneſt glaſs, and the water warmer than the ambient air. He ſays he tried the effect of increaſing the quantity of water in glaſs veſſels of different ſizes, as far as four gallons, without in the leaſt increaſing the ſtroke. He alſo obſerved, that the force of the ſtroke did not increaſe in proportion to the ſize of the globe, or the number of globes employed upon the occaſion; for that he had been as forcibly ſtruck with a phial charged by means of a globe of ſeven inches in diameter, as from one of ſixteen, or from three of ten; and that, at Hamburgh, a ſphere had been employed of a Flemiſh ell in diameter, without the expected increaſe of power. But, in both theſe obſervations, there muſt have been ſome miſtake. He found, that if mercury was uſed inſtead of water, the ſtroke was by no means increaſed in proportion to its ſpecific gravity. He alſo firſt obſerved, that ſeveral men, touching each other, and ſtanding upon electrics, were all ſhocked, though only one touched the gun barrel; but

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[*] Dantzick Memoirs, Vol. iii. p. 504.

[†] Ibid. p. 526.

[‡] Jallabert's Experiences, p. 128.

that no more fire was viſible from them all, than if one had only diſcharged it.

SEVERAL of theſe obſervations ſhow how imperfectly this great experiment was underſtood, for ſome time after it was firſt made. Dr. Watſon, however, obſerved a circumſtance attending the charging of the phial, which, if purſued, would have led him to the diſcovery, which was afterwards made by Dr. Franklin. He ſays, that "when the phial is well electrified, and "you apply your hand thereto, you ſee the fire flaſh from the "outſide of the glaſs, wherever you touch it, and it crackles "in your hand [*]."

HE alſo obſerved, that when a ſingle wire only was faſtened round a phial, properly filled with warm water, and charged; upon the inſtant of its exploſion, the electrical corruſcations were ſeen to dart from the wire, and to illuminate the water contained in the phial.

SEVERAL other very important circumſtances, relating to the diſcharge of the phial, were obſerved by Dr. Watſon. He found that the ſtroke was, *cæteris paribus*, as the points of contact of the non-electrics on the outſide of the glaſs. And upon ſhowing Dr. Bevis the experiments which proved this aſſertion, the Doctor ſuggeſted a more clear and ſatisfactory method of proving it, and which has been the means of accumulating and increaſing the force of charged glaſs, far beyond what was expected from the firſt diſcovery of it. This method was, *coating* the outſide of the phial, very near to the neck, with ſheet-lead, or tinfoil. When a bottle was prepared in this manner, and nearly filled with water, they obſerved, that a perſon who only held in his hand a ſmall wire communicating with that coating, felt as ſtrong a ſhock as he would have felt, if his hand had been in actual contact with every part of the phial touched by the coating*.

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[*] Phil. Tranſ. abridged, Vol. x. p. 298.

Dr. Watson alſo diſcovered, that the electrical power, in the diſcharge of the phial, darts *rectiſſimo curſu*, as he ſtyles it, between the gun-barrel and the phial; and, though it is not ſtrictly true, that the ſhock goes the neareſt way, yet it does ſo *cæteris paribus*, which alone was a conſiderable diſcovery for that time. He obſerved, that, in a company joining hands, a perſon touching two other perſons in the circle, who did themſelves touch one another, felt nothing of the ſhock, his body making no neceſſary part of the circle; and alſo, if a man, holding a wire, which communicated with the outſide of the phial, as it hung upon the conductor, ſhould touch the conductor with it, the exploſion was made, but the man felt nothing†.

In a paper read at the Royal Society, January 21ſt, 1748, Dr. Watſon mentions another diſcovery relating to the Leyden phial, which Dr. Bevis ſuggeſted, and he completed. Having been before fully ſatisfied, that the ſhock from the phial was not in proportion to the quantity of matter contained in the glaſs, but was increaſed by it, and likewiſe by the number of points of non-electric contact on the outſide of the glaſs; he procured three jars, into which he put round leaden ſhot, and, joining their wires and coating, diſcharged them all as one jar. Upon this he obſerved, that the electrical exploſion from two or three of thoſe jars was not double or treble to that from one of them; but that the exploſion from three was much louder than that from two, and the exploſion from two much louder than that from one‡.

This experiment had induced him to imagine, that the exploſion from thoſe jars was owing to the great quantity of non-electric matter contained in them. And whilſt he was conſidering of ſome certain method of aſſuring himſelf whether the fact was ſo, Dr. Bevis informed him, that he had found the electrical exploſion to be as great from covering the ſides of a pane of glaſs, within about an inch of the edge (which was a curious improvement of Mr. Smeaton's), as it could have been from an half pint phial of water. Upon this Dr. Watſon coated large jars with leaf ſilver, both inſide and outſide, within an inch of the top, and from the great exploſion he produced, when ſo little non-electric matter was contained in them, he was of opinion, that the effect of the Leyden bottle was greatly increaſed by, if it was not principally owing to, not ſo much the quantity of non-electric matter contained in the glaſs, as the number of points of non-electric contact within the glaſs, and the denſity of the matter of which thoſe points conſiſted; provided the matter was, in its own nature, a ready conductor of electricity. He alſo obſerved, that the exploſion was greater from hot water incloſed in glaſſes, than from cold; and from his coated jars warmed, than when cold *.

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* Phil. Tranſ. abridged, Vol. x. p. 299.

† Ibid. p. 301.

‡ Ibid. p. 374.

THE Doctor obſerved, that when the circle for the diſcharge was not through perfect conductors, the exploſion was made ſlowly, and not all at once. This law, he ſays, was invariable, but he was not able to account for it. But to prove that the electricity paſſed with its whole force through the circle of non-electrics, he made a circuit conſiſting of iron bars, and ſpoons filled with ſpirits between each bar (but at ſome ſmall diſtance from them), and, upon the exploſion, all the ſpoons were on fire at once. This was the firſt time, as he obſerves, that ſpirits were fired without either the ſpirits, or the non-electric on which they were placed, being inſulated, or put upon original electrics. And yet, he ſays, though we know, from its effects,

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* Phil. Tranſ. abridged, Vol. x. p. 377.

that the electricity goes through the whole circuit of non-electrics, with all its vigour, its progreſs is ſo quick, as not to affect, by attracting or otherwiſe, any light bodies diſpoſed very near the non-electrics, through which it muſt neceſſarily paſs *.

It is curious to obſerve in what manner Dr. Watſon explained the ſhock of the Leyden phial, about the time that he firſt made the experiment with it. He had then been led (by a courſe of experiments which will be mentioned hereafter) to the notion both of the *afflux*, and *efflux* of electric matter in all electrical experiments. To apply this principle to the caſe in hand, he ſuppoſed, that the man who felt the ſhock parted with as much of the fire from his body, as was accumulated in the water and the gun-barrel; and that he felt the effect in both arms, from the fire which was in his body, ruſhing through one arm to the gun barrel, and through the other to the phial. He imagined alſo, that as much fire as the man parted with was inſtantly replaced from the floor of the room, and that with a violence equal to the manner in which he loſt it. It alſo appears, from Dr. Watſon's remarks on ſome ſubſequent experiments of Mr. Monnier, that he then imagined, that though a conſiderable quantity of the electric matter pervaded the glaſs (as he thought was ſeen upon preſenting a non-electric body to it, when it ſtood upon the glaſs ſtand, and without which it could not be charged at all) yet, that the loſs of the electric matter this way was not equal to what came in by the wire; the thinneſs of the glaſs permitting it not wholly, but partially to ſtop the electricity †.

Afterwards, when (from a courſe of experiments, which will alſo be recited in their proper place) Dr. Watſon changed his opinion about this afflux and efflux of electric matter, with a generoſity and frankneſs becoming every inquirer after truth, he retracted this hypotheſis; and, in refutation of it, he farther adds, that the charged phial will explode with equal violence, if the hoop of the wire be bent, ſo as to come near the coating of the phial, without any other non-electric body being near, from which ſuch a quantity could be ſupplied. He had alſo obſerved, that if a man ſtood upon glaſs, and diſcharged the phial, he felt the ſame ſhock as if he had ſtood upon the floor. I ſhall ſubjoin a remarkable paragraph of the Doctor himſelf upon this occaſion, as I think it very applicable even to us, in this more advanced ſtate of the ſcience.

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* Phil. Tranſ. abridged, Vol. x. p. 378.

† Ibid. p. 348.

" I TAKE notice of theſe," ſays the Doctor, " in as much " as, notwithſtanding the very great progreſs which has been " made in our improvements in this part of natural philoſophy, " within theſe few years; poſterity will regard us as only in our " *noviciate*; and therefore it behoves us, as far as we can be " juſtified therein by experiment, to correct any concluſions " we may have drawn, if others yet more probable preſent " themſelves *." The Doctor has lived to ſee, not only that *poſterity* would conſider him and his aſſiſtants at that time, as in their noviciate; but he *himſelf*, already in the courſe of a few years, looks upon both himſelf and them in the ſame light. And, conſidering the quick advances ſtill making in this ſcience, it is to be hoped he may ſtill live to ſee, even the electricians of the preſent year to have been only in their noviciate.

HAVING ſeen what was done by Dr. Watſon towards explaining the electric ſhock, before it was undertaken by Dr. Franklin; let us ſee what obligations we are under to other Engliſh electricians, and particularly Mr. Wilſon.

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* Phil. Tranſ. abridged, Vol. x. p. 373.

Mr. Wilson ſays that, as early as the year 1746, he diſcovered a method of giving the ſhock to any particular part of the body without affecting the reſt [*]. He increaſed the ſtrength of the ſhock by plunging the phial in water, thereby giving it a coating of water on the outſide, as high as it was filled on the inſide [†].

In a letter to Mr. Smeaton, dated Dublin, October 6th, 1746, he mentions his having made ſome experiments, in order to diſcover the law of accumulation of the electric matter in the Leyden bottle; and found, that it was always in proportion to the thinneſs of the glaſs, the ſurface of the glaſs, and that of the non-electrics in contact with the inſide and outſide thereof. The experiments, he ſays, were made with water a little warmed, which was poured into the bottle, while the outſide was immerged in a veſſel filled with water, but a little colder; leaving three inches, or thereabout, uncovered, which was preſerved dry and free from duſt. An account of this experiment he wrote to Mr. Folkes, and it was read before the Royal Society, October 23d, 1746, as appears by their minutes of that day, though the original was loſt or miſlaid.

Another curious experiment Mr. Wilſon made, in order to prove an hypotheſis, which he conceived very early, of the influence of a ſubtle medium ſurrounding all bodies, and reſiſting the entrance or exit of the electric fluid. To determine this, he made the Leyden experiment with a chain, and conſidered each link of it as having two ſurfaces, at leaſt; ſo that lengthening or ſhortening the chain, in each experiment, would occaſion different reſiſtances; and the event, he ſays, proved accordingly. When he made the diſcharge with one wire only, he found the reſiſtance to be leſs than when a chain was uſed. But to leave no room for doubt, he cauſed the chain to be ſtretched with a weight, that the links might be brought nearer into contact, and the event was the ſame as when a ſingle wire had been uſed *.

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[*] Wilſon's Eſſay, p. 88.

[†] Ibid. p. 71.

Two circuits being made, one conſiſting of the arms of a man, and the other of the links of a chain; he found, that the fire would take the arms of the man; but that if the chain were ſtretched, it would take the chain. No perſon, he ſays, who has not made the experiment, would imagine, with how much force the chain muſt be ſtretched before the experiment will anſwer, and the electric fluid paſs through it without producing a ſpark at any of the links; that is, before the links can be brought into abſolute contact with one another, their own weight being by no means ſufficient †.

MR. WILSON obſerved, that if one part of the Leyden phial was ground very thin, and covered with ſealing-wax till it was charged, and then had the ſealing-wax taken off, and a conductor communicating with the earth touched the thin part, the charge would be diſſipated in nearly half the time that it otherwiſe would have been ‡.

HE obſerved that bodies, placed without the electric circuit, would be affected with the ſhock, if they were only in contact with any part of it, or very near it. To ſhew this to the moſt advantage, he ſet a charged phial upon a glaſs ſtand, and placed ſeveral pieces of braſs upon the ſtand, one of them in contact with the chain that formed the circuit, and others a twentieth of an inch from it, or from one another; and, upon making the diſcharge, there was a ſpark viſible between each of them §.

ANALOGOUS, in ſome reſpects, to this, was Mr. Wilſon's obſervation, that if the circuit was not made of metals, or other

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* Letter to Hoadley.

† Wilſon and Hoadley, p. 65.

‡ Wilſon's Eſſay, p. 74.

§ Ibid. p. 90.

very good conductors, the perſon who laid hold of them, in order to perform the experiment, felt a conſiderable ſhock in that arm which was in contact with the circuit.

HE alſo obſerved, that when the phial was coated, within and without, with metals, the firſt exploſion bore the greateſt proportion to the ſubſequent ones, the whole charge being diſſipated almoſt at once; whereas, when water was uſed, the ſubſequent exploſions were more in number, and more conſiderable; and that when the phial was charged with nothing but a wire inſerted into it, the firſt exploſion and the ſubſequent ones were ſtill more nearly equal.

MR. WILSON once happening to break a ſmall wire by the convulſive ſhock given to his arms by the Leyden phial, he faſtened to his hands, well guarded with leather, a large wire, of the thickneſs of a ſlender knitting needle, and placed himſelf in ſuch a manner, that it would neceſſarily be ſtretched, if his arm ſhould be convulſed again. He accordingly diſcharged the phial, and this wire was broken, like the former *.

MR. GEORGE GRAHAM ſhewed how ſeveral circuits for the diſcharge of the Leyden phial might be made at the ſame time, and the fire be made to paſs through them all. He made a number of perſons take hold of a plate of metal, communicating with the outſide of the phial; and all together, likewiſe, laid hold of a braſs rod with which the diſcharge was made; when they were all ſhocked at the ſame time, and in the ſame degree †.

LASTLY, Mr. Canton found, that if a charged phial was placed upon electrics, the wire and the coating would give a ſpark or two alternately; and that, by continuing this operation, the phial would be diſcharged ‡. This diſcovery, which is

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* Wilſon's Eſſay, p. 84.

† Ibid. p. 128.

‡ Ibid. p. 64.

the firſt that I find recorded of this excellent philoſopher, to whom the ſcience of electricity owes ſo much, has a near affinity to the great diſcovery of Dr. Franklin; but he did not then obſerve, that thoſe alternate ſparks proceeded from the two contrary electricities. This hiſtory will furniſh many more inſtances of perſons being on the eve of great diſcoveries, without actually making them. Both Mr. Gralath and Mr. Richman obſerved, in ſeveral caſes, a ſtronger ſpark between two bodies, when both of them were electrified, than when only one of them was in that ſtate; but neither of them ſuſpected that the electricities were of a different kind *.

WE have ſeen what obſervations the Engliſh philoſophers had made upon the Leyden experiment before the time of Dr. Franklin; let us now take a view of what was done by electricians in other parts of the world, within the ſame period.

As Mr. Muſchenbroeck's letter to Mr. Reaumur, concerning the experiment of the phial, came at a time when many learned men were employed about electricity, the Abbé Nollet, and Mr. De Monnier, gentlemen of the academy, zealous to ſearch into ſo an extraordinary a phenomenon, diveſting themſelves of the fear with which the profeſſor's letter had juſtly inſpired them, made the experiment upon themſelves, and, in like manner, ſaid they found the commotion very terrible. The report of it inſtantly ſpread through the court and the city, from whence all ranks of men crouded to ſee this new kind of thunder, and to experience the effect of it †.

THE Abbé Nollet was the firſt who made experiments upon the phial in France, and the reſult of many of them was the ſame with what Dr. Watſon had diſcovered, for which reaſon I ſhall not recite them here. They may all be ſeen at one

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* Wilcke's Preface to the German Verſion of Franklin's Letters.

† Nollet's Leçons de Phyſique, p. 452. Ac. Par. 1746. M. p. 5.

view in his *Leçons de phyſique*, p. 481. The circumſtances which the Engliſh philoſophers had not attended to, are the following.

The Abbé received a ſhock from a bottle out of which the air had been exhauſted, and into which the end of his conductor had been inſerted. As he was amuſing himſelf with obſerving the beautiful irradiations of the electric light in the veſſel, it immediately occurred to him, that, being ſo ſtrongly electrified, it could not fail to give a ſhock ſimilar to that of the Leyden phial; and without any farther reflection, laying one hand on the veſſel, and bringing the other to the conductor, his conjecture was verified, but in a manner that gave him more pain than he could have wiſhed. The blow he received was greater, he ſays, than he ever felt from the Leyden experiment in any other form †.

In the ſame place he obſerves, that he never conſidered the water in the phial as of any uſe, but to convey the electric matter into the inſide of the glaſs; and that he aſcribed the force of the glaſs in giving a ſhock, to that property of it, whereby it retained it more ſtrongly than conductors do, and was not ſo eaſily diveſted of it as they are. The Abbé alſo gave the ſhock with porcelain, and obſerved, that ſome perſons were much more ſenſible to it than others in whatever part of the circuit they were placed *.

Mr. Monnier is ſaid, by Mr. Buffon, to have been the firſt who diſcovered that the Leyden phial would retain its electricity a conſiderable time after it was charged, and to have found it do ſo for thirty-ſix hours, in time of froſt. He frequently electrified his phial at home, and brought it in his hand, through many ſtreets, from the college of Harcourt to his apartments in the King's garden, without any conſiderable diminution of its efficacy [*].

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† Recherches, p. 426.

* Ac. Par. 1746, M. p. 11, 26.

In France as well as in Germany experiments were made to try how many perſons might feel the ſhock of the ſame phial. The Abbé Nollet, whoſe name is famous in electricity, gave it to one hundred and eighty of the guards, in the King's preſence; and at the grand convent of the Carthuſians in Paris, the whole community formed a line of nine hundred toiſes, by means of iron wires between every two perſons (which far exceeded the line of one hundred and eighty of the guards) and the whole company upon the diſcharge of the phial, gave a ſudden ſpring, at the ſame inſtant of time, and all felt the ſhock equally [†].

MR. NOLLET alſo tried the effect of the electric ſhock upon two birds, one of which was a ſparrow, and the other a chaffinch, which, as far as I can find, were the firſt brute animals of any kind that ever received it. The conſequence was, that upon the firſt ſhock, they were both inſtantaneouſly ſtruck motionleſs, and, as it were, lifeleſs, though for a time only; for they recovered ſome few minutes after. Upon the ſecond ſhock, the ſparrow was ſtruck dead, and, upon examination, was found livid without, as if it had been killed with a flaſh of lightning; moſt of the blood veſſels in the body being burſt by the ſhock. The chaffinch revived as before [‡]. Fiſhes were alſo killed with the electric ſhock, by the Abbé, and others.

THE circumſtance of the blood veſſels of the ſparrow being burſt is pretty ſingular. I have ſeen no ſuch effect, when ſmaller animals have been killed by a ſhock fifty times as great as, it is probable, the Abbé uſed upon this occaſion.

THE Abbé Nollet, as well as Mr. Jallabert, mentions the burſting of glaſs veſſels by the electric exploſion. They were pierced, he ſays, with round holes, three or four lines in diameter ‡.

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[*] Phil. Tranſ. abridged, Vol. x. p. 333.

[†] Ibid. p. 335.

[‡] Ibid. p. 336. Ac. Par. 1746. M. p. 32.

It ſeems that the French philoſophers, as well as the Engliſh, had obſerved, that, if the phial ſtood upon glaſs, it could not be charged, except a perſon's hand, or ſome other non-electric ſubſtance were brought near to it. Upon this they imagined the fire ſtreamed out of the hand, and paſſed through the ſubſtance of the phial into the water †. This fact ſurpriſed them very much, as it well might. Mr. Monnier obſerved, that a light body would be attracted by a charged phial, as it ſtood upon the table, and produced a ſpark if any perſon touched the wire; but that the light body muſt be ſuſpended by a non-electric ſubſtance ‡. He likewiſe found, that when the charged phial ſtood upon glaſs, it might be handled with all ſafety §. Theſe experiments ſeem not to have been made with proper circumſpection: for by an attention to theſe very circumſtances, Dr. Franklin was afterwards led to the great diſcovery of the different quality of the electricity, on different ſides of the glaſs.

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* Nollet's Lettres, Vol. i. p. 42.

† Phil. Tranſ. abridged, Vol. x. p. 334.

‡ Ac. Par. 1746. M. p. 684.

§ Phil. Tranſ. abridged, Vol. x. p. 337.

SECTION

SECTION II.

THE METHODS USED BY THE FRENCH AND ENGLISH PHILOSOPHERS, TO MEASURE THE DISTANCE TO WHICH THE ELECTRIC SHOCK CAN BE CARRIED, AND THE VELOCITY WITH WHICH IT PASSES.

WE are now come to an ampler field of electrical experiments, in which we ſhall be ſpectators, not of what might be exhibited in a private room, and by a few operators, but where we ſhall find an amazing apparatus neceſſary, and a great number of aſſiſtants in the management of it; as well as the greateſt judgment, and the moſt unwearied patience in the conduct of it.

THE French philoſophers were the firſt to appear in this field, but they excited the Engliſh to go far beyond them in theſe great undertakings. It has been ſaid already, that a circuit was made of nine hundred toiſes, conſiſting of men holding iron wires betwixt each two, through which the electric ſhock was ſenſibly felt. At another time, they made the ſhock paſs through a wire two thouſand toiſes in length, that is near a Paris league, or about two Engliſh miles and a half; though part of the wires dragged upon wet graſs, went over charmil hedges, or paliſades, and over ground newly ploughed up. Into another chain they took the water of the baſon in the Thuilleries, the ſurface of which was about an acre, and the phial was diſcharged through it*. Mr. Monnier, who made this experiment, mentioning the quantity of ſurface in the baſon of water, as if it was of conſequence to the experiment, and ſaying it was *electrified*, (tho' all he meant by this was, that it received and tranſmitted the electric charge †.) Mr. Gralath made ſeveral experiments, which prove, that bodies which form the circuit of the ſhock are not properly electrified ‡.

Mr. Monnier the younger, alſo endeavoured to determine the velocity of the electric matter; and for this purpoſe, made the ſhock paſs through an iron wire of nine hundred and fifty toiſes in length, but he could not obſerve, that it ſpent a quarter of a ſecond in paſſing it. He alſo found, that when a wire of one thouſand three hundred and nineteen feet, with its extremities brought near together, was electrified; the electricity ceaſed at one end, the moment it was taken off at the other. This fact refuted the opinion of thoſe who maintained, that it was the force of the electrical *ſhock*, which threw the electric matter with ſo great velocity §.

These attempts of the French gave occaſion to the greater, the more accurate, and the more numerous experiments of the Engliſh. The names of the Engliſh gentlemen, animated with a truly philoſophical ſpirit, and who were indefatigable in this buſineſs, deſerve to be tranſmitted to poſterity in every work of this nature.

The principal agent in this great ſcene was Dr. Watſon. He planned and directed all the operations, and never failed to be preſent at every experiment. His chief aſſiſtants were Martin Folkes, Eſq. preſident of the Royal Society, Lord Charles Cavendiſh, Dr. Bevis, Mr. Graham, Dr. Birch, Mr. Peter Daval,

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* Phil. Tranſ. abridged, Vol. x. p. 336.

† Ac. Par. 1746. M. p. 678.

‡ Dantzick Memoirs, Vol. iii. p. 552.

§ Ac. Par. 1746, M. p. 686.

Mr. Trembley, Mr. Ellicott, Mr. Robins, and Mr. Short. Many other perſons, and ſome of diſtinction, gave their attendance occaſionally.

DR. WATSON, who wrote the hiſtory of their proceedings, in order to lay them before the Royal Society, begins with obſerving (what was verified in all their experiments) that the electric ſhock is not, ſtrictly ſpeaking, conducted in the ſhorteſt manner poſſible, unleſs the bodies through which it paſſes conduct equally well; for that, if they conduct unequally, the circuit is always formed through the beſt conductors, though the length of it be ever ſo great.

THE firſt attempt theſe gentlemen made, was to convey the electric ſhock acroſs the river Thames, making uſe of the water of the river for one part of the chain of communication. This they accompliſhed on the 14th and 18th of July 1747, by faſtening a wire all along Weſtminſter bridge, at a conſiderable height above the water. One end of this wire communicated with the coating of a charged phial, the other being held by an obſerver, who, in his other hand, held an iron rod, which he dipped into the river. On the oppoſite ſide of the river, ſtood a gentleman, who, likewiſe, dipped an iron rod in the river, with one hand; and in the other held a wire, the extremity of which might be brought into contact with the wire of the phial.

UPON making the diſcharge, the ſhock was felt by the obſervers on both ſides the river, but more ſenſibly by thoſe who were ſtationed on the ſame ſide with the machine; part of the electric fire having gone from the wire down the moiſt ſtones of the bridge, thereby making ſeveral ſhorter circuits to the phial; but ſtill all paſſing through the gentlemen who were ſtationed on the ſame ſide with the machine. This was, in a manner, demonſtrated by ſome perſons feeling a ſenſible ſhock in their arms and feet, who only happened to touch the wire, at the time of one of the diſcharges, when they were ſtanding upon the wet ſteps which led to the river. In one of the diſcharges made upon this occaſion, ſpirits were kindled by the fire which had gone through the river *.

Upon this, and the ſubſequent occaſions, the gentlemen made uſe of wires, in preference to chains, for this, among other reaſons, that the electricity which was conducted by chains was not ſo ſtrong as that which was conducted by wires. This, as they well obſerved, was occaſioned by the junctures of the links not being ſufficiently cloſe, as appeared by the ſnapping and flaſhing at every juncture, where there was the leaſt ſeparation. Theſe leſſer ſnappings, being numerous in the whole length of a chain, very ſenſibly leſſened the great diſcharge at the gun-barrel.

Their next attempt was to force the electrical ſhock to make a circuit of two miles, at the New River at Stoke Newington. This they performed on the 24th of July 1747, at two places; at one of which the diſtance by land was eight hundred feet, and by water two thouſand: in the other, the diſtance by land was two thouſand eight hundred feet, and by water eight thouſand. The diſpoſition of the apparatus was ſimilar to what they before uſed at Weſtminſter bridge, and the effect anſwered their utmoſt expectations. But, as in both caſes, the obſervers at both extremities of the chain, which terminated in the water, felt the ſhock, as well when they ſtood with their rods fixed into the earth twenty feet from the water, as when they were put into the river; it occaſioned a doubt, whether the electric circuit was formed through the windings of the river, or a much ſhorter way, by the ground of the meadow: for the experiment plain-

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* Phil. Tranſ. abridged, Vol. x. p. 394.

ly ſhewed, that the meadow-ground, with the graſs on it, conducted the electricity very well.

BY ſubſequent experiments, they were fully convinced, that the electricity had not, in this caſe, been conveyed by the water of the river, which was two miles in length, but by land, where the diſtance was only one mile; in which ſpace, however, the electric matter muſt neceſſarily have paſſed over the New River twice, have gone through ſeveral gravels pits, and a large ſtubble field *.

JULY 28th, they repeated the experiment, at the ſame place, with the following variation of circumſtances. The iron wire was, in its whole length, ſupported by dry ſticks, and the obſervers ſtood upon original electrics; the effect of which was, that they felt the ſhock much more ſenſibly than when the conducting wire had lain upon the ground, and when the obſervers had likewiſe ſtood upon the ground, as in the former experiment.

AFTERWARDS, every thing elſe remaining as before, the obſervers were directed, inſtead of dipping their rods into the water, to put them into the ground, each one hundred and fifty feet from the water. They were both ſmartly ſtruck, though they were diſtant from each other above five hundred feet †.

THE ſame gentlemen, pleaſed with the ſucceſs of their former experiments undertook another, the object of which was, to determine, whether the electric virtue could be conveyed through dry ground; and, at the ſame time, to carry it through water to a greater diſtance than they had done before. For this purpoſe, they pitched upon Highbury-barn beyond Iſlington, where they carried it into execution on the 5th of Auguſt 1747. They choſe a ſtation for their machine, almoſt equally diſtant

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* Phil. Tranſ. abridged, Vol. x. p. 360.

† Ibid. p. 357.

from two other ſtations for obſervers upon the New River; which were ſomewhat more than a mile aſunder by land, and two miles by water. They had found the ſtreets of London, when dry, to conduct very ſtrongly, for about forty yards; and the dry road at Newington about the ſame diſtance. The event of this trial anſwered their expectations. The electric fire made the circuit of the water, when both the wires and the obſervers were ſupported upon original electrics, and the rods dipped into the river. They alſo both felt the ſhock, when one of the obſervers was placed in a dry gravelly pit, about three hundred yards nearer the machine than the former ſtation, and one hundred yards diſtant from the river: from which the gentlemen were ſatisfied, that the dry gravelly ground had conducted the electricity as ſtrongly as water.

From the ſhocks which the obſervers received in their bodies, when the electric power was conducted upon dry ſticks, they were of opinion, that, from the difference of diſtance ſimply conſidered, the force of the ſhock, as far as they had yet experienced, was very little, if at all impaired. When the obſervers ſtood upon electrics, and touched the water, or the ground, with the iron rods, the ſhock was always felt in their arms or wriſts; when they ſtood upon the ground with their iron rods, they felt the ſhock in their elbows, wriſts, and ancles; and when they ſtood upon the ground without rods, the ſhock was always felt in the elbow and wriſt of that hand which held the conducting wire, and in both ancles *.

The laſt attempt of this kind which theſe gentlemen made, and which required all their ſagacity and addreſs in the conduct of it, was to try whether the electric ſhock was perceptible at twice the diſtance to which they had before carried it, in ground

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* Phil. Tranſ. abridged, Vol. x. p. 360.

perfectly dry, and where no water was near; and alſo to diſtinguiſh, if poſſible, the reſpective velocity of electricity and ſound.

FOR this purpoſe, they fixed upon Shooter's-hill, and made their firſt experiments on the 14th of Auguſt 1747, a time, when, as it happened, but one ſhower of rain had fallen during five preceding weeks. The wire communicating with the iron rod, which made the diſcharge, was ſix thouſand ſeven hundred and thirty-two feet in length, and was ſupported all the way upon baked ſticks; as was alſo the wire which communicated with the coating of the phial, which was three thouſand eight hundred and ſixty-eight feet long, and the obſervers were diſtant from each other two miles. The reſult of the exploſion demonſtrated, to the ſatisfaction of the gentlemen preſent, that the circuit performed by the electric matter was four miles, viz. two miles of wire, and two of dry ground, the ſpace between the extremities of the wires; a diſtance which, without trial, as they juſtly obſerved, was too great to be credited. A gun was diſcharged at the inſtant of the exploſion, and the obſervers had ſtop watches in their hands, to note the moment when they felt the ſhock: but, as far as they could diſtinguiſh, the time in which the electric matter performed that vaſt circuit might have been inſtantaneous *.

IN all the exploſions where the circuit was made of conſiderable length, it was obſerved, that though the phial was very well charged, yet that the ſnap at the gun barrel, made by the exploſion, was not near ſo loud as when the circuit was formed in a room; ſo that a by-ſtander, ſays Dr. Watſon, though verſed in thoſe operations, would not imagine, from ſeeing the flaſh, and hearing the report, that the ſtroke, at the extremity of the

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* Phil. Tranſ. abridged, Vol. x. p. 363.

conducting wire, could have been conſiderable; the contrary whereof, when the wires were properly managed, he ſays, always happened.

Still the gentlemen, unwearied in theſe purſuits, were deſirous, if poſſible, to aſcertain the abſolute velocity of electricity at a certain diſtance; becauſe though, in the laſt experiment, the time of it's progreſs was certainly very ſmall, if any, they were deſirous of knowing, ſmall as that time might be, whether it was meaſureable, and Dr. Watſon had contrived an excellent method for that purpoſe.

Accordingly, on the 5th of Auguſt 1748, the gentlemen met once more, and the laſt time, at Shooter's-hill; when it was agreed to make an electric circuit of two miles, by ſeveral turnings of the wire, in the ſame field. The middle of this circuit, they contrived to be in the ſame room with the machine, where an obſerver took in each hand one of the extremities of the wires, each of which was a mile in length. In this excellent diſpoſition of the apparatus, in which the time between the exploſion and the ſhock might have been obſerved to the greateſt exactneſs, the phial was diſcharged ſeveral times; but the obſerver always felt himſelf ſhocked at the very inſtant of making the exploſion. Upon this the gentlemen were fully ſatisfied, that through the whole length of this wire, which was 12276 feet, the velocity of the electric matter was inſtantaneous *.

These experiments excited the admiration of all foreign electricians. Profeſſor Muſchenbroeck, who was greatly ſatisfied with the extent and ſucceſs of them, ſaid, in a letter to Dr. Watſon, upon the occaſion, *Magnificentiſſimis tuis experimentis ſuperaſti conatus omnium.*

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* Phil. Tranſ. abridged, Vol. x. p. 368.

IT is ſaid by ſome, that the laſt of theſe experiments go upon a wrong ſuppoſition, and therefore can be of no uſe; it being ſuppoſed that the very ſame particles of the electric fluid, which were thrown on one ſide of the charged glaſs, actually made the whole circuit of the intervening conductors, and arrived at the oppoſite ſide: whereas Dr. Franklin's theory only requires that the deficiency on one ſide of the glaſs be ſupplied from the neighbouring conductors; which may, in return, receive as much as they parted with, from the ſide of the glaſs that was overcharged. So that, to be a little more particular, the redundancy of electric matter on the charged ſide of a pane of glaſs, only paſſes into the bodies which form that part of the circuit which is contiguous to it, driving forward that part of the fluid which was natural to them; till, at length, the fluid which reſided in thoſe conductors which formed the laſt part of the circuit, paſſes into the exhauſted ſide of the glaſs.

BUT ſhould this be the caſe (though in great diſcharges it ſuppoſes the natural quantity of electricity in bodies to be very conſiderable) and ſhould Dr. Watſon, and other philoſophers at that time, have conceived otherwiſe; it does not follow, that the experiments could poſſibly determine *nothing*: for there ſtill remains ſomething to be meaſured, viz. the time required for the ſucceſſive diſlodging the electric fluid in the whole length of the circuit.

WERE the whole maſs of the electric matter contained in all the intervening conductors abſolutely ſolid, no motion could be made at one extremity, without producing an inſtantaneous motion at the other; juſt as if one end of a rod be ſtruck, the motion is inſtantly communicated to the other end. But this cannot be the caſe in an elaſtic medium, the parts of which yield to one another. In this caſe, the motion is communicated in a real ſucceſſion, like a vibration, running the whole length of the circuit; which muſt therefore take up time, and be meaſurable. The motion of ſound may be meaſured, though no particle of the vibrating air be finally diſplaced. Theſe great experiments of Dr. Watſon, therefore, had a real object, only it appeared to be too ſmall to be aſcertained by them.

SECTION III.

Miscellaneous discoveries of Dr. WATSON, and others, before the time of Dr. FRANKLIN.

THE firſt of theſe diſcoveries in order of time, and in importance ſecond to none (except that of the ſhock itſelf, and Dr. Franklin's diſcovery of the different electricity of the oppoſite ſides of the charged glaſs) was that of Dr. Watſon, proving, that the glaſs tubes and globes did not contain the electric power in themſelves, but only ſerved as *firſt movers*, and *determiners*, as he calls it, of that power.

He was firſt led to this diſcovery by obſerving, that, upon rubbing the glaſs tube, while he was ſtanding upon cakes of wax (in order, as he expected, to prevent any of the electric power from diſcharging itſelf through his body upon the floor) the power was, contrary to his expectation, ſo much leſſened, that no ſnapping could be obſerved upon another perſon's touching any part of his body; but that if a perſon not electrified held his hand near the tube, while it was rubbed, the ſnapping was very ſenſible *.

The event was the ſame when the globe was whirled in ſimilar circumſtances. For if the man who turned the wheel, and who, together with the machine was ſuſpended upon ſilk, touched the floor with one foot, the electric fire appeared upon the conductor; but if he kept himſelf free from any communication with the floor, no fire was produced.

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* Phil. Tranſ. abridged, Vol. x. p. 303.

Dr. Watson by this, and the following experiments in conjunction, diſcovered, what he calls, the complete circulation of the electric matter. He obſerved, that only a ſpark or two would iſſue from his hand to the inſulated machine, unleſs he, at the ſame time, formed a communication between the conductor and the floor; but that then there was a conſtant and copious flux of the electric matter to the machine.

Observing, that while his hand was in contact with the conductor, the man who turned this inſulated machine gave ſparks, which would fire inflammable ſubſtances, and perform other electrical experiments which were uſually performed at the conductor; he naturally imagined, that the fire iſſued from the man, for the very ſame reaſon that all electricians had before imagined that it came from the conductor; and ſeeing that the man gave no fire unleſs there was a communication between the floor and the conductor, he concluded that, in this caſe, the fire was ſupplied by that communication, ſo that the courſe of the electricity was inverted, as he expreſſes it *.

It was not then ſuſpected, that the eye could not diſtinguiſh in what direction an electric ſpark proceeds. Electricians naturally imagined that all electric powers, and conſequently the electric fluid, which they ſuppoſed to be the cauſe of theſe powers, exiſted in the excited electric, whatever it was; and that whatever powers were exerted by electrified bodies, proceeded from a real communication of electric matter to them. Accordingly, when Dr. Watſon found that, by cutting off the communication of the electric with the floor, all

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* Phil. Tranſ. abridged, Vol. x. p. 305.

electrical operations were ſtopped, he concluded, that the electric fluid was collected from the floor to the rubber, and thence conveyed to the globe. For the ſame reaſon, ſeeing the rubber, or the man who had a communication with it, give no ſparks but when the conductor was connected with the floor, he would as naturally conclude that the globe was ſupplied from the conductor, as he had before concluded that it was ſupplied from the rubber.

Comparing both theſe experiments together, Dr. Watſon was led to infer, that, in all electrical operations, there was both an *afflux* of electric matter to the globe, and the conductor, and likewiſe an *efflux* of the ſame electric matter from them *.

Finding that a piece of leaf ſilver was ſuſpended between a plate electrified by the conductor, and another communicating with the floor, he reaſons from it in the following manner. "No body can be ſuſpended in equilibrio but by the joint "action of two different directions of power: ſo here, the blaſt "of electric ether from the excited plate blows the ſilver "towards the plate unexcited, and this laſt, in its turn, by the "blaſt of electric ether from the floor ſetting through it, drives "the ſilver towards the plate electrified. We find from "hence, likewiſe, that the draught of electric ether from the floor "is always in proportion to the quantity thrown by the globe "over the gun barrel, or the equilibrium by which the ſilver is "ſuſpended could not be maintained †."

Dr. Watson obſerves, that the Abbé Nollet, two years before he made this communication, had given it as his opinion (though without any experiment which proved it) that the electric matter did not only proceed from the electrified bodies, but from all others about them, to a certain diſtance ‡.

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* Phil. Tranſ. abridged, Vol. x. p. 311.

† Ibid. p. 310.

‡ Ibid. p. 315.

Some time after this, Dr. Watſon obſerves, in a paper read at the Royal Society, January 21ſt, 1748, that Dr. Bevis had carried his experiment, to prove that rubbing the tube or the globe, only conveyed, and did not produce the electric matter, farther than he had done. For he had obſerved, above a year before, that placing one man upon electrics, to rub the tube or globe, and another alſo upon electrics to touch them, as the conductor; both the man who rubbed, and the man who touched the excited glaſs would give a ſpark; and farther, that if they touched one another, the ſnapping was much greater than if either of them touched a perſon ſtanding upon the floor. Upon this the Doctor ſeems to have corrected his former opinion of the afflux and efflux of electric matter: for he accounts for this fact by ſuppoſing, that as much electricity as was taken from the perſon rubbing was given to him who touched the conductor, being conveyed by the globe. By this means the electricity of the former of theſe perſons, he obſerves, was more rare than it naturally was, and that of the latter more denſe; ſo that the denſity of electricity between theſe two perſons differed more than that between either of them, and another perſon ſtanding upon the floor. In this manner did Dr. Watſon diſcover, what Dr. Franklin obſerved, about the ſame time, in America, and called the *plus* and *minus* in electricity[*].

Dr. Watson obſerved that the flame at the end of an electrified wire was ſenſible to the hand, as a cool blaſt of wind, and that when light ſubſtances were attracted and repelled between an electrified plate and one communicating with the floor, the ſucceſſion of theſe alternate attractions and repulſions was extremely quick, ſo that ſometimes the eye could hardly keep pace with it; and that when a glaſs globe, of about an inch in diameter, very light and finely blown, was put upon a plate of metal, and another plate hung on the conductor over it, the ſtrokes from the alternate attractions and repulſions were almoſt too quick for the ear. From this laſt experiment he likewiſe deduced an argument to prove the extreme velocity with which theſe glaſs globes were attracted and repelled. He ſays, that if they were let fall from the height of ſix feet or more upon a wooden floor, or even a plate of metal, they were rarely broken; but that by the attraction and repulſion of them between theſe plates, though at the diſtance of no more than one ſixth of an inch, they were frequently beaten to pieces [*].

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[*] Phil. Tranſ. abridged, Vol. x. p. 369.

THE Doctor alſo proved, that the electric matter paſſed through the ſubſtances of the metal of communication, and not over the ſurface of it, by covering a wire with a mixture of wax and roſin, and diſcharging a phial through it.

I MUST here obſerve, that Mr. Monnier the younger diſcovered, that electricity is not communicated to homogeneous bodies in proportion to their maſſes or quantity of matter, but rather in proportion to their ſurfaces; and yet that all equal ſurfaces do not receive equal quantities of electricity, but that thoſe receive the moſt which are moſt extended in length; that a ſquare ſheet of lead, for inſtance, received a much leſs quantity of electricity than a ſmall ſtrip of the ſame metal with a ſurface equal to that of a ſquare ſheet [†].

MR. WILSON, whoſe curious obſervations on the Leyden phial have been mentioned in a former ſection, claims no ſmall ſhare of honour in this. As early as the latter end of the year 1746, he made the ſame diſcovery that Dr. Watſon had done, viz. that the electric fluid did not come from the globe, but from the earth itſelf, and from all other non-electric bodies about the apparatus. He ſuggeſted a method of proving this in a letter to Mr. Ellicott from Cheſter; and mentions his having completed the experiment himſelf ſoon after, in a letter to Mr. Smeaton, from Dublin.

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[*] Phil. Tranſ. abridged, Vol. x. p. 309.

[†] Ibid. p. 338.

Having conceived that the difference between electric and non-electric bodies was owing to the different reſiſtance which a *ſubtle medium*, as he calls it, on the ſurfaces of all bodies gave to the paſſage of the electric fluid; and conceiving that heat would rarify this medium, and thereby convert electrics into non-electrics, he made ſome experiments which confirmed him in that ſuppoſition. He found that one perſon might communicate electricity to another, notwithſtanding the intervention of a conſiderable quantity of red-hot glaſs. He alſo made other experiments of a ſimilar nature, as diſcharging phials by means of hot glaſs, hot amber, and various other heated electrics. Theſe, however, as Mr. Canton afterwards obſerved, might be owing to the hot air upon the ſurfaces of thoſe bodies, which he found to tranſmit electricity very well. But another experiment, which Mr. Wilſon made upon melted roſin, does not ſeem liable to that objection. He poured the melted roſin into a phial, and found that he could give ſhocks with it; but he obſerved, that theſe ſhocks diminiſhed as the roſin grew cold, and that when it was quite cold, they entirely ceaſed *.

Mr. Wilson mentions a curious experiment (of which, however, he does not ſay that he was the inventor) which he made with paper vanes ſtuck in a cork, and ſuſpended by a magnet. Theſe, he ſays, if they were brought near the point of any body proceeding from the prime conductor, would turn round very ſwiftly, but would not turn at all in vacuo. This blaſt he thought was occaſioned by the iſſuing of the electric matter out of the point, which cauſed a current in the air; but he did not try what would be the conſequence of preſenting the vanes to a point which received the electric fluid *.

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* Wilſon's Eſſay, p. 143.

Lastly, Mr. Wilſon obſerved, that if a needle were preſented to a piece of down hanging to the conductor, it would cling cloſe to it; but that, upon preſenting any thing that was blunt, it would be repelled again; and ſays that Mr. Canton made ſeveral curious experiments of the ſame kind †.

Mr. Smeaton, within this period, obſerved, that if a man who was inſulated preſſed againſt the globe with the flat part of his hand, while another perſon, ſtanding on the floor, did the ſame, in order to excite it, the perſon who was inſulated would hardly be electrified at all; but that, if he only laid his fingers lightly on the globe, he would be electrified very ſtrongly ‡. The ſame ingenious perſon alſo obſerved, that upon heating the middle of a large bar of iron to a glowing heat, and electrifying it, the electric power of the part that was heated was as ſtrong as that of the cold part §.

For ſeveral curious diſcoveries relating to electricity, made within this period, we are indebted to Dr. Miles. In a paper read at the Royal Society, January 25th, 1746, he ſays, that having excited a ſtick of black ſealing-wax with white and brown paper, or clean dried flannel, he was able to kindle common lamp-ſpirits with it. Comparing the ſtick of wax with the glaſs tube, he obſerved a remarkable difference between the appearance of fire from both, though he did not underſtand the reaſon of it. He ſays he found the luminous effluvia to proceed in a much greater quantity from the top of

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* Wilſon's Eſſay, p. 141.

† Ibid.

‡ Ibid. p. 153.

§ Ibid. p. 129.

his finger to the ſtick of wax, than they did to the glaſs. He ſeveral times obſerved a ſmall globular ſpot of fire to appear firſt on his finger, from which iſſued regular ſtreams towards the wax, in the form of a comet's tail. This is now well known to be the conſtant appearance of the electric fire between an unelectrified body and an electric excited negatively *.

Dr Miles found a ſtick of ſulphur to perform very well; but not at all, when he had put an iron rammer in the center of it, to ſtrengthen it. It is remarkable, that after ſetting this ſtick upright in a cupboard, it loſt all its electric virtue, and could never afterwards be excited in the leaſt degree. This effect the Doctor attributed to its being put up without any cover.

Dr. Miles alſo mentions his having got a tube of green glaſs, which he could never excite but with great difficulty, and then but to a ſmall degree †.

The ſame ingenious gentleman, ſome time after, made an experiment upon pieces of leaf braſs in a bottle hermetically ſealed. To theſe he found he could give motion by the approach of the excited tube, in the ſame manner as if they had been in the open air; but one appearance ſtruck him, of which he by no means gives a ſatisfactory account. He obſerved that when he removed the tube from the exhauſted glaſs ſlowly, no commotion was ſeen in the leaf braſs, but a very briſk one upon removing it ſuddenly. Indeed this fact could not have been underſtood but by comparing it with other facts depending upon the ſame principle, and which were not diſcovered till ſome years after ‡.

From England, to which, as an Engliſhman, I would give the preference only in matters of abſolute indifference, I paſs over to France, where, next to thoſe made in England, the moſt important diſcoveries, and greateſt number of them were made

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* Wilſon's Eſſay, p. 317.

† Phil. Tranſ. abridged, Vol. x. p. 320.

‡ Ibid. p. 326.

made in the period of which I am treating. And, without all diſpute, the greateſt name in France, in this or any other period, except that of Mr. Du Fay, his friend and aſſociate, is that of the Abbé Nollet.

THE favourite obſervation of Mr. Nollet, on which he built his darling theory of affluences and effluences was, that bodies not inſulated, plunged in electric atmoſpheres, ſhewed ſigns of electricity. He obſerved a ſenſible blaſt from the hand of a perſon not electrified, in the above mentioned circumſtances, alſo the attraction and repulſion of light bodies by them, the appearance of flame, the diminution of their weight by increaſed evaporation and perſpiration, and almoſt every other appearance and effect of electricity. Moreover obſerving that his globe contracted a foulneſs while it was whirling, even when rubbed with a clean hand, he had the curioſity to collect a quantity of the matter which formed that foulneſs; and finding that, when it was put into the fire, it had the ſmell of burnt hair, he concluded that it was an animal ſubſtance; and that it had been carried by the affluent electricity from his own body to the globe *.

THE only miſtake of this ingenious philoſopher in theſe experiments, and which was the ſource of many others, which, in the end, greatly bewildered and perplexed him, was, that the electricity of the body, which was plunged in the atmoſphere of an electrified body, was of the ſame nature with that of the electrified body. Had he but preſerved the diſtinction, which Mr. Du Fay had diſcovered, between the two electricities, and imagined that the body electrified, and that which was plunged in its atmoſphere were poſſeſſed of theſe two different and oppoſite electricities, he might have been led to the great diſcoveries

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* Nollet's Recherches, p. 142.

made by Mr. Canton, Dr. Franklin, and Mr. Wilcke; which, we ſhall find, aroſe from that ſingle obſervation; and he would have avoided a great deal of debate and contention, which has not ended to his advantage.

This partial diſcovery of Mr. Nollet is by no means the only one of his, that the hiſtory of electricity preſents in this period. He made ſeveral experiments on pointed bodies, and obſerved, that thoſe which had the ſmalleſt points ſooneſt threw out bruſhes of electric light, but did not ſhow other ſigns of electricity ſo ſtrong as bodies that were not pointed *.

He took a great deal of pains in making experiments, in order to determine the degree in which different ſubſtances conducted the electric fluid; and found that the ſmoke of gum lac, turpentine, karabé, and ſulphur did not carry away the electricity of an excited tube ſo ſoon as the ſmoke of linen, wood, and more eſpecially the ſteam of water, and the effluvia of burning tallow, and of other fatty ſubſtances. In ſhort, he found, that vapours which were not watery did very little, or no injury to electrical experiments, provided the tube was not expoſed to them near the fire which cauſed them. A ſmoky room did not prevent his performing experiments, at leaſt in any great degree; nor were odoriferous effluvia at all prejudicial to them †.

Several curious obſervations were made by the Abbé upon heat, and heated bodies. He found, that a piece of iron glowing hot, ſo as to throw off ignited particles, did not leave the ſmalleſt trace of electricity in an excited tube, to which it had been brought within five or ſix inches, and only held there two or three ſeconds; but it ceaſed to affect the tube at the ſame diſtance before it ceaſed to be red, and had no influence at all long before it was cold. The electricity of the tube, in this inſtance, was

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* Recherches, p. 146.

† Ibid. p. 194, &c.

probably conveyed through the air heated by the iron; as it can hardly be ſuppoſed, that the iron emitted any effluvia capable of producing that effect *.

HE found that the excited tube loſt nothing of its electricity in the focus of a burning mirror. That the flame of a candle, or the near approach of it, would deſtroy electricity had been known before: he obſerved, that the flame was ſenſibly diſturbed by the approach of the excited tube, and he mentions Mr. Du Tour, and the Abbé Needham's having found, that the interpoſition of the thinneſt piece of glaſs, or of any other ſubſtance, between the candle and the tube prevented the diſſipation of the electricity. From this fact it was inferred, that the diſſipation was owing to ſome effluvia proceeding from the candle †.

CONTINUING his obſervations on what increaſed or impeded electrical experiments, he found, that a light body, placed on a non-electric ſtand, moved more briſkly upon the approach of an electrified body, than when it was placed upon an electric ſtand ‡. Several electrical experiments, he obſerved, ſucceeded beſt when there was a number of ſpectators preſent, and when they drew near, and ſtood cloſe together to ſee his experiments; provided they did not occaſion ſo great a perſpiration as made his glaſſes moiſt §. This obſervation we ſhall find accounted for hereafter by Mr. Wilcke.

THE Abbé moiſtened, with water or ſpirit of wine, a ſlender and pointed bar of iron, and thought that the blaſt from the point of it was more ſenſible than when it was not moiſtened; which he attributed to the electric fluid carrying away with it ſome of the particles of the water, and of the ſpirit of wine ‖.

SOME few obſervations the Abbé made on the difference between excited and communicated electricity, and between the

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* Recherches, p. 216.
† Ibid. p. 219.
‡ Ibid. p. 122.
§ Ibid. p. 123.
‖ Ibid. p. 140.

electricity of glaſs and that of ſulphur. He obſerved, that the electricity of an excited globe or tube, cauſed an odd ſenſation upon the face, as if a ſpider's web were drawn over it; whereas that effect was ſeldom produced by communicated electricity. Excited electricity, he alſo ſays, might be perceived by the ſmell, at more than a foot diſtance, when communicated electricity could not [*].

He melted ſulphur in a glaſs globe, by turning it over a chafing diſh of burning coals; when he obſerved, that ſmall pieces of ſulphur, before they were melted, were attracted and repelled by the glaſs within, at the ſame time that the aſhes of the coals were attracted without [†]. Holding a piece of excited ſulphur in one hand, with a piece of down ſticking to it, and ready to fly off, the down, he ſays, would cling faſt to the ſulphur, upon preſenting to it an excited glaſs tube, which he held in his other hand [‡].

I shall, in the laſt place, recite the Abbé Nollet's experiments made in vacuo. He found that glaſs, and other electrics, might be excited in vacuo, but not ſo ſtrongly as in the open air [§]. He obſerved that there was a remarkable difference between the appearance of the electric light in vacuo, and in the open air; being much more diffuſe, and unbroken in vacuo [||]. Inſerting the extremity of his conductor into an exhauſted glaſs veſſel, he obſerved the veſſel to be full of light, whenever he brought his hand to it; that the light was conſiderably increaſed when he ſpread his hand over it; and that when a ſpark was taken from the conductor, the whole veſſel ſeemed to be full of light. He alſo obſerved, that ſmall pieces of metal, incloſed in the veſſel, adhered cloſe to the glaſs; but detached themſelves from it, on the approach of the finger, or of any conductor on the outſide.

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[*] Recherches, p. 136.
[†] Ibid. p. 184.
[‡] Ibid. p. 124.
[§] Ibid. p. 236.
[||] Ibid. p. 243.

M. MONNIER attempted, in an ingenious method, to be certain whether the quantity of electricity communicated to a body was in proportion to its ſolid or ſuperficial contents. He firſt found that an anvil which weighed two hundred pounds, gave but an inconſiderable ſpark, while the ſpark from a ſpeaking-trumpet of tin, which weighed but ten pounds, but was eight or nine feet long, was almoſt equal to the ſhock of the Leyden phial. He then obſerved, that a ball of lead four inches in diameter, gave a ſpark of the ſame force with another from a thin piece of lead of the ſame ſuperficies, in the form of a hoop. And, laſtly, he took a thin and long piece of lead, and obſerved, that when it was electrified in its whole length, it gave a very ſtrong ſpark, but a very ſmall one when it was rolled into a lump. But, becauſe a ſquare piece of lead did not give a ſpark equal to one from a piece of the ſame quantity of ſurface, but of greater length, he concluded that, though electrification is ſtronger in proportion to the ſurfaces of electrified bodies, yet that, of equal ſurfaces, that which is drawn out to the greateſt length will have the advantage*.

THERE are a few other names of electricians in France, whoſe experiments and obſervations, made within this period, deſerve to be mentioned. Of theſe is Mr. Boulanger. He took great pains to determine the degree in which different ſubſtances are capable of being excited. The experiments, he ſays, were made with the greateſt care: and though the ſtate of the ſcience did not admit of this buſineſs being determined with greater accuracy, it may not be diſagreeable to ſee the reſult of them; which he has compriſed in the following table, beginning with thoſe that are leaſt excitable in every column.

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* Ac. Par. 1746. M. p. 693.

FIRST COLUMN.

Ebony.
Guiacum.
Box wood.
Sandal wood.
Oak.
Elm.
Aſh.
Linden tree.
Roſe.
Willow.
Ozier.
Cork.
Dry wood of all kinds.
All dry plants.

SECOND COLUMN.

Shells of all kinds.
Whalebone.
Bones.
Ivory.
Horn.
Scales.
Parchment.
Hair.
Wool.
Feathers.
Cotton.
Silk.

THIRD COLUMN.

Alum.
Sugar Candy.
The Phoſphorus of Berne.
Yellow and white wax.
Japan varniſh.
Sandarac.
Maſtich.
Amber.
Jet.
Pitch.
Gum copal.
Gum lac.
Colophonia.
Sulphur.
Sealing-wax.
All ſalts which have ſufficient conſiſtence.
All reſins.

FOURTH COLUMN.

Loadſtone.
Hand-ſtone.
Marble of all colours.
Slate.
Free-ſtone.
Granite.
Porphyry.
Jaſper.

Varniſhed earth.
Cornelians.
Agates.
All opaque precious ſtones.
Porcelaine.

FIFTH COLUMN.

Hyacinth.
Opal.
Emerald.
Amethyſt.
Topaz.
Ruby.
Sapphire.
Cat's eye.
Peridote.
Granite.
Rock cryſtal.
Venice and Muſcovy talc.
Coloured diamonds, eſpecially yellow.
White diamonds, eſpecially the brilliant.
All tranſparent precious ſtones.
Glaſs, and all vitrifications without excepting thoſe of metals.

THE inference which this author draws from this catalogue is, that the moſt brittle, and the moſt tranſparent ſubſtances, are always the moſt electric; and he has recourſe to an awkward hypotheſis to account for the marcaſites not being excitable at all, notwithſtanding they are both brittle and tranſparent. He ſays it is owing to condenſed air contained in thoſe ſubſtances, which is known to prevent excitation [*].

THE ſame author ſays, that mineral waters are much more ſenſibly affected with electricity than common water; that black ribbons are much ſooner attracted than thoſe of other colours; and, next to them, the brown, and deep red [†].

MR. LE CAT, a phyſician at Rouen, who has diſtinguiſhed himſelf by ſeveral performances in the learned world, ſuſpended ſeveral pieces of leaf gold at his conductor, and obſerved that they hung at different diſtances, according to their ſizes, the

[*] Boulanger, p. 74.

[†] Ibid. p. 124.

ſmaller pieces placing themſelves nearer the conductor, and the larger receding farther from it. This he compares to the diſtances at which the planets make their revolutions round the ſun, and he ſuppoſed the cauſe to be the ſame in both. The ſame author very particularly compares the electric ſhock, which had juſt been diſcovered, to thunder *.

GERMANY affords but few articles for the electrical hiſtory of this period; one of them, however, is curious, and well deſerves to be tranſmitted to poſterity. Mr. Gordon of Erford excited the electricity of a cat ſo ſtrongly, that, when it was communicated by iron chains, it fired ſpirit of wine †.

IT has been mentioned before, that ſeveral gentlemen in Germany, as well as in England, had found, that if the man who rubbed the globe ſtood upon electrics, ſparks were perceived upon touching him; but Mr. Klingenſtierna, a Swede, and Mr. Stroema, were the firſt who properly electrified by the rubber; and their experiments were publiſhed in the Acts of the Royal Academy of Sciences at Stockholm for the year 1747 ‡.

MR. JALLABERT, profeſſor of philoſophy at Geneva, found that a coating of pitch did not prevent the conductor from being electrified, which proved that the electric fluid enters the ſubſtance of metals. He alſo proved, that ice was a conductor of electricity, by making the Leyden experiment with a bottle in which water was frozen §.

THE amazing and extenſive effects of electricity now began to make philoſophers look for it where it had not been ſuſpected before. The firſt account that is given of woollen garments being obſerved to exhibit ſigns of electricity, when they were put off, after the flaſhes of light they gave were known to be owing to electricity, was ſent to the Royal Society by Mr.

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* Hiſtoire, p. 84—85.

† Nollet's Recherches, p. 98.

‡ Wilcke, p. 112.

§ Hiſtoire, p. 95, 96.

Coke of the Iſle of Wight, who ſays, that a lady of his acquaintance obſerved it; and that it was alſo at laſt found, that it was only new flannel, and after ſome time wearing, which gave that appearance, and that this property was loſt when it was waſhed *.

THE ſame appearance, he obſerves, upon another occaſion, was moſt conſpicuous in froſty weather; in which ſeaſon he takes notice, that there is generally, not only a greater purity of the air, and abſence of moiſture, but that all hairy and horny ſubſtances (for hairs, as he ſays, are only ſmall horns) are more elaſtic, and conſequently ſuſceptible of, and more capable of exciting ſtrong vibrations. He ſays, that the flannel being rendered damp with ſea water, and afterwards dry, would heighten the electric appearances †.

BUT though this was the firſt appearance of the kind that was obſerved, after it was known to ariſe from electricity, ſimilar appearances had been ſeveral times noted before. Bartholin, who flouriſhed in 1650, wrote a book *De luce animalium*, in which he ſuppoſes, that unctuous effluvia had a great ſhare in thoſe appearances. The ſame writer ſays, that Theodore Beza might be ſeen by a light proceeding from his eye-brows; and that ſparks would flaſh from the body of Charles Gonzaga, Duke of Mantua, upon being gently rubbed. But he does not ſay whether he had any particular hairy, or ſcaly ſuperficies to his ſkin ‡.

DR. SIMPSON, who publiſhed a philoſophical diſcourſe on fermentation, dedicated to the Royal Society, in 1675, alſo takes notice of the light proceeding from animals on frication, or pectation as he calls it, and inſtances in the combing of a woman's head, the currying of a horſe, and the ſtroking of a cat's back §.

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* Phil. Tranſ. abridged, Vol. x. p. 343.

† Ibid. p. 343.

‡ Ibid. p. 344.

§ Ibid. p. 357.

Mr. Clayton alſo, in a letter to Mr. Boyle, dated June 23d, 1684, at James-town in Virginia, gives him an account of a ſtrange accident, as he calls it, which happened to one Mrs. Sewall, whoſe wearing apparel emitted a flaſhing of ſparks, which were ſeen by ſeveral perſons. The like happened to Lady Baltimore her mother-in-law *.

I shall conclude this ſection with what I can find, in this period, about increaſing the power of electricity, and meaſuring its effects.

Mr. Monnier the younger, whoſe name has been frequently mentioned in the courſe of this hiſtory, uſed glaſs ſpheroids inſtead of globes, and endeavoured to increaſe his electrical power by uſing ſeveral of theſe ſpheriods at a time; but he found, upon trial, that they did not anſwer his expectations; and was thence diſpoſed to conclude, that there might be a *ne plus ultra* in the intenſity of electricity, as well as in the heat communicated to boiling water †.

The power of glaſs in electrifying being found to be ſo great, it is no wonder that philoſophers ſhould endeavour to find what kind of glaſs was capable of being excited to the greateſt degree. Among other propoſals we find a very memorable one communicated to the Royal Society, April 6th, 1749, by Mr. Boze. He ſays, that a glaſs ball which has often been employed in violent diſtillations, and other chemical operations ſends forth electricity incomparably more ſtrong, than any glaſs which had never been expoſed to ſo violent a fire. This article is the more curious, as it ſhews us how much philoſophers at this time piqued themſelves upon diſcoveries in electricity. He aſſerts his being the firſt perſon who ever mentioned this notable circumſtance, as he calls it, and deſires Dr. Watſon, to whom he communicated it, to let him have the honour of that improvement in the Philoſophical Tranſactions *.

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* Phil. Tranſ. abridged, Vol. x. p. 278.

† Ibid. p. 330.

IT was within this period that Dr. Watſon contrived to improve the ſtrength of electricity by moiſtening the rubber of his globe, though he was not aware of all the reaſons for it. He obſerved that the man who ſtood on the floor, to excite the globe by his hand, did it more ſtrongly than a cuſhion. This, he ſays, he could not conceive to be owing to any other difference, than to his hand being more moiſt, and conſequently more readily conducting the electricity from the floor; wherefore he ordered his machine, and even his cuſhion, to be made damp; and then found that the electricity was as ſtrong as when the globe was rubbed by the hand †.

A GENTLEMAN at Chartres in France, greatly increaſed the effects of electricity by means of moiſture, for aſſerting which he is very much ridiculed by the author of *Hiſtoire de l'Electricité.*

MR. WILSON ſays, that if the cuſhion (which he made of leather) was gilt with ſilver, braſs, or copper, it would do very well; and that the ſilk line on which the conductor hung ſhould be red or yellow ‡. The table, he ſays, ſhould ſtand on moiſt ground, or a wire paſs from the machine to the moiſt ground §.

DR. WATSON alſo found, that though no electricity could be produced by rubbing the globe with original electrics perfectly dry, yet that they anſwered very well when they had been made moiſt; the water imbibed by thoſe ſubſtances ſerving as a canal of communication to the electricity between the hand, or the cuſhion, and the globe; in the ſame manner as the air, replete with vapours in damp weather, prevents the accumulation of the electric matter in any conſiderahle degree, by conducting it as faſt as it is excited to the neareſt non-electrics. He obſerved,

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* Phil. Tranſ. abridged, Vol. x. p. 329.

† Ibid. p. 312.

‡ Wilſon's Eſſay, p. 5, 6.

§ Ibid. p. 8.

ſerved, on the contrary, that moſt vegetable ſubſtances, though made as dry as poſſible, furniſhed electricity, but ſmall in quantities. He excited electricity not only from linen, cotton, &c. but even from ſheet lead and a deal board *.

The Abbé Nollet ſays, that he found oil of turpentine upon a piece of woollen cloth excited glaſs very powerfully, but that the leaſt water mixed with it prevented the excitation †.

Mr. Boulanger ſays, that if two cylinders be made of the ſame kind of glaſs, and of the ſame faſhion, one of them tranſparent, and the other tinged with any colour, the tranſparent cylinder will be excited more eaſily than the coloured one ‡. He acknowledges, however, that ſometimes the moſt tranſparent, and the moſt brittle glaſs is capable of acquiring but little electricity §. In another place he ſays, that a cylinder of three or four lines in thickneſs will acquire a ſtronger, and a more laſting electricity than a cylinder of one line thick ‖. He alſo ſays, that a perſon's two hands, or one cuſhion, is better than more ⊥.

About the ſame time that Dr. Watſon made his firſt experiments upon the Leyden phial, Mr. Canton diſcovered a method by which the quantity of electricity accumulated in the phial might be meaſured to a good degree of exactneſs. He took the charged phial in his hand, and made it give a ſpark to an inſulated conductor, which ſpark he took off with his other hand. This operation he repeated till the whole was diſcharged, and he eſtimated the height of the charge by the number of the ſparks. This is a pretty certain and exact method of knowing how high a phial *has been* charged; but what electricians chiefly want is a method of aſcertaining how high a phial *is* charged, or the exact force of the charge while it is contained in the glaſs.

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* Wilſon's Eſſay, p. 380.

† Recherches, p. 168.

‡ Boulanger, p. 64.

§ Ibid. p. 164.

‖ Ibid. p. 135.

⊥ Ibid. p. 136.

SOMETHING of this kind was done by Mr. Ellicott, in the ſame year 1746. He propoſed to eſtimate the ſtrength of common electrification, by its power to raiſe a weight in one ſcale of a balance, while the other ſhould be held over the electrified body, and pulled to it by its attractive power *. Mr. Gralath alſo conſtructed an electrometer upon the ſame principle †.

THE Abbé Nollet applied the threads that Mr. Grey and Du Fay had uſed in electrical experiments, to ſhew the degree of electricity. He hung two of them together, and obſerved the angle of their divergence, by means of the rays of the ſun, or the light of a candle, and their ſhadow upon a board placed behind them. Mr. Waitz alſo thought of the ſame kind of electrometer, with this improvement, that he loaded the ends of the threads with ſmall weights ‡.

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* Boulanger, p. 324.

† Dantzick Memoirs, Vol. i. p. 525.

‡ Hiſtoire, p. 58.

SECTION IV.

EXPERIMENTS ON ANIMAL, AND OTHER ORGANIZED BODIES IN THIS PERIOD; AND OTHER EXPERIMENTS CONNECTED WITH THEM, MADE CHIEFLY BY THE ABBE NOLLET.

HITHERTO the effect of electricity upon human bodies had not been attended to, farther than the mere ſhock of the Leyden phial. But we ſhall now ſee a curious ſet of experiments on this ſubject exhibited by the Abbé Nollet. The Engliſh philoſophers, who led the way in almoſt every other application of electricity, were among the laſt to try its effects upon animals, and other organized bodies. The only article that I can find upon this ſubject, before the diſcoveries of the Abbé Nollet, is one of Mr. Trembley's; who ſays that ſeveral perſons had obſerved, that while they were electrified, their pulſe beat a little faſter than before. He ſays, that he himſelf, after having been electrified a long time together, had felt an odd ſenſation all over his body, and that ſome perſons had felt very ſharp pains after being electrified *.

THE ingenious Abbé Nollet begins his experiments with the evaporation of fluids by electricity. They were made with the greateſt attention, and the following obſervations were the reſult of them.

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* Phil. Tranſ. abridged, Vol. x. p. 321.

"1. ELECTRICITY augments the natural evaporation of "fluids; ſince, excepting mercury, which is too heavy, and "the oil of olives, which is too viſcous, all the others which "were tried ſuffered a diminution which could not be aſcribed "to any other cauſe than electricity.

"2. ELECTRICITY augments the evaporation of thoſe fluids "the moſt, which are moſt ſubject to evaporate of themſelves. "For the volatile ſpirit of ſal ammoniac ſuffered a greater loſs than "ſpirit of wine, or turpentine; theſe two more than common "water; and water more than vinegar, or the ſolution of nitre.

"3. ELECTRICITY has a greater effect upon fluids when "the veſſels which contain them are non-electrics; the effects "always ſeeming to be a little greater when the veſſels were "of metal, than when they were of glaſs.

"4. THIS increaſed evaporation was more conſiderable when "the veſſel which contained the liquor was more open, but the "effects did not increaſe in proportion to their apertures. For "when theſe liquors were electrified in veſſels, whoſe aperture "was four inches in diameter, though they preſented to the air "a ſurface ſixteen times larger than when they were contained "in veſſels whoſe aperture was one inch in diameter, they "were, nevertheleſs, far from ſuffering a diminution proportioned "to that difference.

"5. ELECTRIFICATION does not make any liquors evaporate "through the pores, either of metal, or of glaſs; ſince "after experiments which were continued ten hours, there was "found no diminution of their weight, when the veſſels in "which they were contained were well ſtopped*."

AFTER having made experiments on fluids, the Abbé began another courſe on ſolids of various kinds, the reſult of which was, that they loſt weight only in proportion to the moiſture they contained, and the openneſs of their pores *.

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* Nollet's Recherches, p. 327.

The Abbé alſo extended his experiments to other ſenſible qualities of bodies, as their ſmell, their taſte, and chemical properties; but found no change in any of them, after a ſtrong and continued electrification of a variety of ſubſtances. Electrification did not affect the power of the magnet, and neither retarded nor accelerated the heating or cooling of bodies †.

He then proceeded to the electrification of capillary tubes, full of water; it having been obſerved by Mr. Boze, who communicated the obſervation to Mr. Nollet ‡, that the water would iſſue in a conſtant ſtream when they were electrified; whereas it would only drop very ſlowly without that operation. Every perſon, at firſt ſight, would judge that the ſtream was accelerated, and that the electrified veſſel would ſoon be empty: but this accurate philoſopher was unwilling to rely on firſt appearances, and therefore reſolved to aſcertain the fact, by meaſuring the time, and the quantity of liquor running out. And, in order to know if the acceleration, ſuppoſing there were any, was uniform, during the whole time of the running out, he made uſe of veſſels of different capacities, terminating in pipes of different bores, from three lines in diameter, to the ſmalleſt capillaries.

As the Abbé did not find it ſo eaſy a matter to draw a ſafe concluſion in this caſe as might at firſt be imagined, he gives us in groſs the following reſult of above an hundred experiments §.

"1. The electrified ſtream, though it divides, and carries "the liquid farther, is neither ſenſibly accelerated nor retarded, "when the pipe through which it iſſues is not leſs than a line "in diameter.

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* Nollet's Recherches, p. 335.

† Ibid. p. 341.

‡ Ibid. p. 343.

§ Ibid. p. 327. Phil. Tranſ. abridged, Vol. x. p. 382.

" 2. Under this diameter, if the tube is wide enough to let " the liquid run in a continued ſtream, electricity accelerates it " a little; but leſs than a perſon would imagine, if he judged " by the number of jets which are formed, and by the diſtance " to which they go.

" 3. If the tube be a capillary one, from which the water " only drops naturally, the electrified jet not only becomes a " continued ſtream, and even divided into ſeveral ſtreams, but " is alſo conſiderably accelerated; and the ſmaller the capillary " tube is, the greater, in proportion, is this acceleration.

" 4. So great, is the effect of the electric virtue, that it drives " the water in a conſtant ſtream out of a very ſmall capillary " tube, out of which it had not before been able even to drop."

The moſt unaccountable of theſe experiments, as the ingenious Abbé acknowledges, are thoſe which ſuppoſe a retardation of the electrified current, and he long doubted the fact; but a great number of experiments, carefully noted in his journal, obliged him to admit it, though ſtill with heſitation, and to account for it in the beſt manner he could; which, indeed, is not very ſatisfactory [*].

The beautiful appearance of theſe ſtreams of electrified water, when the experiment was exhibited in the dark, is particularly deſcribed by this author, after Meſſrs. Boze and Gordon, who firſt obſerved it [†].

These laſt experiments ſerved as a baſis to the Abbé's future inquiries. He conſidered all organized bodies as aſſemblages of capillary tubes, filled with a fluid that tends to run through them, and often to iſſue out of them. In conſequence of this idea, he imagined, that the electric virtue might poſſibly communicate ſome motion to the ſap of vegetables, and alſo augment the inſenſible perſpiration of animals. He began with the following experiments, the reſult of which confirmed his ſuppoſition *.

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[*] Recherches, p. 351.

[†] Ibid. p. 354.

He electrified for four or five hours together, fruits, green plants, and ſpunges, dipped in water which he had carefully weighed; and found that, after the experiment, all thoſe bodies were remarkably lighter than others of the ſame kind, weighed with them, both before and after the experiment, and kept in the ſame place and temper †.

The electrification of growing vegetables was firſt begun in Britain. Mr. Maimbray at Edinburgh electrified two myrtle trees, during the whole month of October 1746; when they put forth ſmall branches and bloſſoms ſooner than other ſhrubs of the ſame kind, which had not been electrified. Mr. Nollet, hearing of this experiment, was encouraged to try it himſelf ‡.

He took two garden pots, filled with the ſame earth, and ſowed with the ſame ſeeds. He kept them conſtantly in the ſame place, and took the ſame care of them; except that one of the two was electrified fifteen days together, for two or three, and ſometimes four hours a day. The conſequence was, that the electrified pot always ſhewed the ſprouts of its ſeeds two or three days ſooner than the other. It alſo threw out a greater number of ſhoots, and thoſe longer in a given time; which made him believe, that the electric virtue helped to open and diſplay the germs, and thereby to facilitate the growth of plants. This, however, our cautious philoſopher only calls a conjecture, which required farther confirmation. The ſeaſon, he ſays, was then too far advanced to allow him to make as many experiments as he could have wiſhed, but he ſays the next courſe

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* Recherches, p. 355.

† Phil. Tranſ. abridged, Vol. x. p. 383.

‡ Recherches, p. 356.

of experiments had greater certainty, and they are not leſs intereſting *.

The ſame experiments were carrying on about the ſame time by Mr. Jallabert, Mr. Boze, and the Abbé Menon, principal of the college of Bueil at Angers, who all drew the ſame concluſions from them †.

The Abbé choſe ſeveral pairs of animals of different kinds, cats, pigeons, chaffinches, ſparrows, &c. All theſe he put into ſeparate wooden cages, and weighed them. One of each pair he electrified for five or ſix hours together, and then weighed them again. The reſult was, that the electrified cat was commonly ſixty-five or ſeventy grains lighter than the other, the pigeon from thirty-five to thirty-eight grains, the chaffinch or ſparrow ſix or ſeven grains. In order to have nothing to charge upon the difference that might ariſe from the temperament of the individuals he happened to pitch upon, he repeated the ſame experiments, by electrifying that animal of each pair which had not been electrified before; and, notwithſtanding ſome ſmall varieties the electrified animal was conſtantly lighter than the other in proportion ‡.

After theſe experiments, he had no doubt but that electricity increaſed the inſenſible perſpiration of animals, but it was not certain whether this increaſe was in the ratio of their bulks, or in that of their ſurfaces. The Abbé's opinion was, that it was neither in the one, nor the other, ſtrictly ſpeaking, but in a ratio much more nearly approaching the latter than the former; ſo that he imagined, there was no room to apprehend, that a human perſon electrified would loſe near a fiftieth part of his weight, as it appeared to him to have happened to one ſort of bird; nor 140th part, as to the pigeon, &c. All that he had

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* Recherches, p. 358, &c. Phil. Tranſ. abridged, Vol. x. p. 383.

† Recherches, p. 357.

‡ Ibid. p. 366, &c.

then obſerved upon that head was, that a young man or woman, between the ages of twenty and thirty, from being electrified five hours together, had loſt ſeveral ounces of their weight, more than they were wont to loſe when they were not electrified [†].

The Abbé obſerves, that no inconvenience whatever was felt by the perſons who ſubmitted to be electrified in this manner. They only found themſelves a little exhauſted, and had got a better appetite. He adds, that none of them found themſelves ſenſibly warmer, and that he could not perceive that their pulſe was encreaſed [†].

These laſt experiments on human bodies, he juſtly obſerves, are difficult to purſue with exactneſs, becauſe the cloathing, which cannot ſtrictly be compared to the hairs or feathers of animals, retains a conſiderable ſhare of the perſpired matter, and prevents our forming a good judgment of the whole effect of the electric virtue.

The foregoing experiments, he ſays, convinced him of the reality of the *effluent* matter carrying away with it the perſpirable parts of bodies, and what could be evaporated from their ſurfaces. And he was convinced of the *affluent* matter, by obſerving all thoſe effects produced, if, inſtead of electrifying bodies themſelves, they were only brought near a large body which was electrified. He moiſtened a thick ſpunge in water, and cut it into two pieces, and then weighed the parts ſeparately, and placing the whole near a large electrified body; he found that, after an electrification of five or ſix hours, that part of the ſpunge which was nearer to the electrified body had loſt more weight than the other. From this fact he concluded, that if any part of an animal body was preſented to a large lectrified ſubſtance, it would perſpire more than the other, and that

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[*] Phil. Tranſ. abridged, Vol. x. p. 384. Recherches, p. 382.

[†] Recherches, p. 389.

perhaps obſtructions might by this means be removed from the pores of it *.

The experiments above recited of Mr. Nollet by no means ſatisfied the Engliſh philoſophers, and particularly Mr. Ellicott, who made experiments to refute the theory which the author had deduced from them. He obſerved that the ſyphon, though electrified, would only deliver the water by drops, if the baſon in which the water was contained was electrified too. But this does not invalidate Mr. Nollet's curious experiments upon the ſubject of evaporation and perſpiration. For when an animal body is electrified, there is always non-electric matter enough in the atmoſphere, to anſwer the purpoſe of the unelectrified baſon, in the experiment of the capillary tube; thereby to cauſe a continual exhalation of the perſpirable matter from the pores of the ſkin. Beſides, the capillary tube will, in fact, unite the water in a conſtant ſtream, when it has only the open air to throw it into. In all debates upon ſubjects in natural philoſophy, facts ought only to be oppoſed to facts. The veracity of the Abbé Nollet is not to be called in queſtion; though it muſt be acknowledged, that, in his later writings, at a time when his favourite ſyſtem was in danger, he makes miſtakes with reſpect to the facts that nearly affect it.

To account for the appearance of light, which ſeems, in ſome caſes, to iſſue from a non-electric body preſented to an excited electric, and which Mr. Nollet thought to be the affluent matter, Mr. Ellicott ſuppoſes that it was the light which had come from the electric. In accounting for the ſuſpenſion of leaf gold between an electrified and an unelectrified plate, Mr. Ellicott's theory made it neceſſary to ſuppoſe (what Dr. Franklin afterwards found not to be fact) that the leaf gold will al-

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* Phil. Tranſ. abridged, Vol. x. p. 385.

ways be ſuſpended nearer the unelectrified than the electrified plate.

IN his anſwer to Mr. Nollet, Mr. Ellicott alſo endeavours to account for the electric matter iſſuing from a point at the extremity of the conductor, more ſenſibly than if it had terminated round or flat. He ſays that the effluvia, in ruſhing from the globe along the conductor, as they approached the point, were brought nearer together, and therefore were denſer there than in any part of the rod. Conſequently, he ſays, if the light be owing to the denſity and velocity of the effluvia, it will be viſible at the point, and no where elſe. This, as far as I can find, was the firſt attempt to account for this phenomenon; but it by no means accounts for the whole virtue of the conductor being diſſipated from ſuch points. Indeed, it is no wonder that the influence of points which are but imperfectly underſtood even at this day, furniſhed too difficult a problem ſo many years ago*.

IT will, now, be univerſally acknowledged, that there was very great merit in theſe experiments of the Abbé Nollet, made upon animal and other organized bodies. He opened a new and noble field of electrical diſcoveries, and he purſued them with great attention, perſeverance, and expence. This laſt circumſtance, I ſuppoſe, may have been the reaſon why his experiments have not, as far as I can find, been reſumed and purſued by any electrician ſince his time, though there ſeems to be great room to improve upon what he began. The only method in which they can be conducted to any purpoſe, would be by the help of a machine for perpetual electrification, to go by wind or water; which would, likewiſe, ſerve for many other capital experiments in electricity. This application of electricity, in particular, may perhaps be of more uſe in medicine, than any other mode in which it has hitherto been adminiſtered.

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* Phil. Tranſ. abridged, Vol. x. p. 393.

MR. JALLABERT of Geneva carried the experiments on plants farther than the Abbé Nollet had done; and, by electrifying bottles in which the plants were growing in water, and placing in the ſame expoſure other bottles, containing plants of the ſame kind; he proved, in the cleareſt manner, that the electrified plants always grew faſter, and had finer ſtems, leaves, and flowers than thoſe which were not electrified, and conſumed more of their water *.

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* Beccaria dell' elettriciſmo naturale et artificiale, p. 125.

SECTION V.

THE HISTORY OF THE MEDICATED TUBES, AND OTHER COMMUNICATIONS OF MEDICINAL VIRTUES BY ELECTRICITY; WITH THEIR VARIOUS REFUTATIONS.

IN the courſe of this hiſtory we have ſeen frequent inſtances of ſelf-deception, for want of perſons attending to all the eſſential circumſtances of facts; but nothing we have yet ſeen equals what was exhibited in the years 1747 and 1748. Mr. Grey's deceptions were chiefly owing to his miſtaking the cauſe of real appearances; but in this caſe we can hardly help thinking, that, not only the imagination and judgment, but even all the external ſenſes of philoſophers muſt have been impoſed upon. It was aſſerted by Signior Pivati at Venice (who has all the merit of theſe extraordinary diſcoveries), and, after him, by Mr. Verati at Bologna, Mr. Bianchi at Turin, and Mr. Winckler at Leipſick, that if odorous ſubſtances were confined in glaſs veſſels, and the veſſels excited, the odours and other medicinal virtues would tranſpire through the glaſs, infect the atmoſphere of the conductor, and communicate the virtue to all perſons in contact with it; alſo that thoſe ſubſtances, held in the hands of perſons electrified, would communicate their virtues to them; ſo that medicines might be made to operate without being taken into the ſtomach. They even pretended to have wrought many cures by the help of electricity applied this way. Some of the more curious of theſe pretended experiments deſerve to be recorded, for the entertainment and inſtruction of poſterity.

THE forementioned Signior Johannes Franciſco Pivati, a perſon of eminence at Venice, ſays, in an Italian epiſtle, printed at Venice with all the uſual licences, in the year 1747, that a manifeſt example of the virtue of electricity was ſhown in the balſam of Peru, which was ſo concealed in a glaſs cylinder, that, before the excitation of it, not the leaſt ſmell could by any means be diſcovered. A man who, having a pain in his ſide, had applied hyſſop to it, by the advice of a phyſician, approached the cylinder thus prepared, and was electrified by it. The conſequence was, that when he went home, and fell aſleep, he ſweated, and the power of the balſam was ſo diſperſed, that even his cloaths, the bed, and chamber, all ſmelled of it. When he had refreſhed himſelf by his ſleep, he combed his head, and found the balſam to have penetrated his hair; ſo that the very comb was perfumed *.

THE next day, Signior Pivati ſays, he electrified a man in health in the ſame manner, who knew nothing of what had been done before. On his going into company half an hour afterwards, he found a gradual warmth diffuſing itſelf through his whole body, and he grew more lively and chearful than uſual. His companion was ſurpriſed at an odour, and could not imagine whence it proceeded, but he himſelf perceived that the fume aroſe from his own body, at which he alſo was much ſurpriſed, not having the leaſt ſuſpicion that it was owing to the operation which had been performed upon him by Signior Pivati †.

MR. WINCKLER of Leipſick, being ſtruck with ſo extraordinary a relation, ſays, that he was deſirous of trying the power of

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* Phil. Tranſ. abridged, Vol. x. p. 400.

† Ibid. p. 401.

electricity on certain ſubſtances in the ſame manner, and that he found the event to confirm what had been related *.

He put ſome pounded ſulphur into a glaſs ſphere, ſo well covered and ſtopped, that, on turning it over the fire, there was not the leaſt ſmell of ſulphur perceived. When the ſphere was cold he electrified it; and, immediately, a ſulphureous vapour iſſued from it, and, on continuing the electricity, filled the air, ſo as to be ſmelled at the diſtance of more than ten feet. He called in a friend well verſed in electricity, profeſſor Haubold, and ſeveral others, as witneſſes and judges of this fact; but they were preſently driven away by the ſtench of the ſulphur. He ſtaid a little longer himſelf in this ſulphureous atmoſphere, and was ſo impregnated thereby, that his body, cloaths, and breath retained the odour even the next day. On repeating this experiment in the preſence of a perſon who was converſant with the effects of ſulphur, the ſigns of an inflamed blood were viſible in his mouth on the third day †.

After this he tried the effect of a more agreeable ſmell, and filled the ſphere with cinnamon. When he had heated this as before, the ſmell of cinnamon was ſoon perceived by the company, and the whole room was in a ſhort time ſo perfumed by it, that it immediately affected the noſes of all who came in, and the odour remained the next day.

He tried the balſam of Peru with the like ſucceſs, when his above mentioned friend (whoſe teſtimony, he ſays, he did not care to be without) after he had received the power of the balſam, ſmelled ſo ſtrong of it, that going abroad to ſupper, he was often aſked by the company what prefume he had about him. The next day, when Mr. Winckler was drinking tea,

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* Phil. Tranſ. abridged, Vol. x. p. 401.

† Ibid.

he ſays, he found an unuſual ſweet taſte, owing to the fumes of the balſam that ſtill remained in his mouth *.

In a few days, when the ſphere had loſt all the ſcent of the balſam, they let a chain out of the chamber window, and extended it through the open air, into another room detached from the former. Here they ſuſpended the chain on ſilken lines, and gave it into the hand of a man, who alſo ſtood on extended ſilken lines, and knew nothing of their purpoſe. When the electricity had been excited for ſome time, the man was aſked whether he ſmelled any thing; and, on ſnuffing up his noſe, he ſaid he did. Being aſked again what ſmell it was, he ſaid he did not know. When the electrification had been continued for about a quarter of an hour, the room ſmelled ſo ſtrong of it, that the man, who knew nothing of the balſam, ſaid his noſe was filled with a ſweet ſmell, like that of ſome ſort of balſam. After ſleeping in a houſe, a conſiderable diſtance from the room where the experiment was tried, he roſe very chearfully in the morning, and found a more pleaſant taſte than ordinary in his tea †.

I SHALL only give an account of two inſtances of the effect of medicine applied in this manner. The aſſiſtance of Signior Pivati, the celebrated inventor of this improvement in electricity, was implored by a young gentleman, who was miſerably afflicted by a quantity of corrupted matter collected in his foot, that eluded all the attempts of the phyſicians. Signior Pivati filled a glaſs cylinder with proper materials; and, having electrified it, drew ſparks from the part affected, and continued the operation for ſome minutes. When the patient went to bed, he had a good night, and a mitigation of his pain. When he awaked in the morning, he found a ſmall red tubercle on his

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* Phil. Tranſ. abridged, Vol. x. p. 401.

† Ibid.

foot, which only itched, as if a cold humour had flowed through the inner part of his foot. He ſweated every night for eight days together, and at the end of that time was perfectly well.

After this, Signior Donadoni, biſhop of Sebenico, came to Signior Pivati, attended by his phyſician and ſome friends. His lordſhip was at that time ſeventy-five years old, and had been afflicted with pains in his hands and feet for ſeveral years. The gout had ſo affected his fingers, that he was not able to move them; and his legs, ſo that he could not bend his knees. In this deplorable ſituation, the poor old biſhop intreated Signior Pivati to try the effects of electricity on him. The electrician undertook it, and proceeded after the following manner. He filled a glaſs cylinder with diſcutient medicines, and managed it ſo, that the electric virtue might enter into the patient, who preſently felt ſome unuſual commotions in his fingers; and the action of electricity had been continued but two minutes, when his lordſhip opened and ſhut both his hands, gave a hearty ſqueeze to one of his attendants, got up, walked, ſmote his hands together, helped himſelf to a chair, and ſat down, wondering at his own ſtrength, and hardly knowing whether it was not a dream. At length he walked out of the chamber down ſtairs, without any aſſiſtance, and with all the alacrity of a young man*.

A variety of facts of this nature being publiſhed, and ſeemingly well atteſted, engaged all the electricians of Europe to repeat theſe experiments; but none of them could ſucceed but thoſe mentioned above. An excellent remark of Mr. Baker's, who adviſed trying all theſe experiments, notwithſtanding their ſeeming very improbable, deſerves to be quoted here. "Romantic as theſe things may ſeem, they ſhould not be ab-

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* Philoſophical Tranſactions abridged, Vol. x. p. 403.

" ſolutely condemned without a fair trial, ſince we all, I be-
" lieve remember the time, when thoſe phenomena in electrici-
" ty, which are now the moſt common and familiar to us,
" would have been thought deſerving as little credit as the caſes
" under conſideration may ſeem to do, had accounts of them been
" ſent to us from Rome, Venice, or Bologna, and had we
" never experienced them ourſelves*."

To ſee theſe wonders, and to be aſſured of their truth or fallacy, Mr. Nollet, who was deeply intereſted in every thing that related to his favourite ſtudy, and who ſet no bounds to his labour or expences, in the purſuit of truth, even paſſed the Alps, and travelled into Italy, where he viſited all the gentlemen who had publiſhed any account of theſe experiments. But though he engaged them to repeat their experiments in his preſence, and upon himſelf; and though he made it his buſineſs to get all the beſt information he could concerning them, he returned, convinced that the accounts of cures had been much exaggerated; that in no one inſtance had odours been found to tranſpire through the pores of excited glaſs; and that no drugs had ever communicated their virtues to perſons who only held them in their hands while electrified.

He had no doubt, however, but that, by continued electrification, without drugs, ſeveral perſons had found conſiderable relief in various diſorders; particularly, that a paralytic perſon had been cured at Geneva, and that a perſon deaf of one ear, a footman who had a violent pain in his head, and a woman who had a diſorder in her eyes, were cured at Bologna †.

The Engliſh philoſophers ſhowed no leſs attention to this ſubject than the Abbé Nollet. The Royal Society had received an account from Mr. Winckler of his experiments, to prove the tranſudation of odoriferous matter through the pores of excited glaſs; and none of them ſucceeding here, the ſecretary was deſired to write to Mr. Winckler, in the name of the ſociety, deſiring him to tranſmit to them, not only a circumſtantial account of his manner of making the experiments, but likewiſe ſome globes and tubes, fitted up by himſelf, for that purpoſe.

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* Phil. Tranſ. abridged, Vol. x. p. 406.

† Ibid. p. 413, &c.

These veſſels, and directions how to uſe them, Mr. Winckler actually ſent, and the experiments were made with every poſſible precaution, at the houſe of Dr. Watſon (the moſt intereſted and active perſon in the kingdom in every thing relating to electricity) on the 12th of June 1751. There were preſent Martin Folkes, preſident of the Royal Society, Nicholas Mann, Eſq. vice preſident, Dr. Mortimer, and Peter Daval, Eſq. ſecretaries, Mr. Canton a fellow, and Mr. Shroder, a gentleman of diſtinction, well known to, and correſponding with Mr. Winckler. But, notwithſtanding all the pains theſe gentlemen took, purſuing, with the utmoſt exactneſs, the directions of Mr. Winckler, and alſo uſing methods of their own, which they thought ſtill better adapted to force the effluvia through the glaſs, they were unſucceſsful. They were not able to verify Mr. Winckler's experiments even in one ſingle inſtance*.

But perhaps the moſt ſatisfactory refutation both of this pretended tranſudation of odours, and the medicinal effects of electricity above mentioned, was made at Venice, the very place where this medical electricity took its riſe. The experiments were made by Dr. Bianchini, profeſſor of medicine, in the preſence of a great number of witneſſes, many of them prejudiced in favour of the pretended diſcoveries; but they were all forced to be convinced of their futility, by the evidence of facts, and by experiments made with the greateſt care and accuracy [*].

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* Phil. Tranſ. abridged, Vol. xlvii. p. 321.

AFTER the publication of theſe accounts properly atteſted, every unprejudiced perſon was ſatisfied, that the pretended diſcoveries from Italy and Leipſick, which had raiſed the expectation of all the electricians in Europe, had no foundation in fact; and that no method had yet been diſcovered whereby the power of medicine could by electricity be made to inſinuate itſelf into the human body [†].

LASTLY, I would obſerve, that Dr. Franklin alſo ſhowed, by ſeveral experiments, the impoſſibility of mixing the effluvia or virtue of medicines with the electric fluid [‡].

IN ſome reſpects ſimilar to the experiments with the *medicated tubes* (as thoſe mentioned above were uſually called) was that of profeſſor Boze, which he termed the *beatification*; and which, for a long time, employed other electricians to repeat after him, but to no purpoſe. His deſcription of this famous experiment was, that if, in electrifying, large globes were employed, and the electrified perſon were placed upon large cakes of pitch, a lambent flame would by degrees ariſe from the pitch, and ſpread itſelf round his feet; and that from thence it would be propagated to his knees and body, till, at laſt, it aſcended to his head; that then, by continuing the electrification, the perſon's head would be ſurrounded by a glory, ſuch a one, in ſome meaſure, as is repreſented by painters in their ornamenting the heads of ſaints [§].

THIS experiment, as well as that of the medicated tubes, ſet all the electricians in Europe to work, and put them to a great deal of expence; but none of them could ſucceed, ſo as to produce an appearance any thing like that deſcribed by Mr. Boze.

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[*] Phil. Tranſ. abridged, Vol. xlviii. p. 399.

[†] Ibid. p. 406.

[‡] Franklin's Letters, p. 82.

[§] Phil. Tranſ. abridged, Vol. x. p. 411.

No perſon took more pains in this buſineſs than Dr. Watſon. He himſelf underwent the operation ſeveral times, ſupported by ſolid electrics three feet high. Upon being electrified very ſtrongly, he found, as he ſays ſeveral other perſons alſo did, a tingling upon the ſkin of his head, and in many parts of his body, or ſuch a ſenſation as would be felt from a vaſt number of inſects crawling over him at the ſame time; and he conſtantly obſerved the ſenſation to be the greateſt in thoſe parts of his body which were neareſt to any non-electric, but ſtill no light appeared upon his head, though the experiment was ſeveral times made in the dark, and with ſome continuance *.

At length the Doctor, wearied with theſe fruitleſs attempts, wrote to the profeſſor, and his anſwer ſhowed that the whole had been a mere trick. He candidly acknowledged, that he had made uſe of a ſuit of armour, which was decked with many bullions of ſteel, ſome pointed like nails, ſome like wedges, and ſome pyramidal; and that when the electrization was very vigorous, the edges of the helmet would dart forth rays, ſomething like thoſe which are painted on the heads of ſaints. And this was all his boaſted beatification †.

This ſame Mr. Boze, who ſeems to have had a ſingular affectation of ſomething myſterious and marvellous in his experiments, in a letter to the Royal Society at London, ſaid that he had been able, by electricity only, to invert the poles of a natural magnet, to deſtroy their virtue, and to reſtore it again, but he did not deſcribe his method ‡. Conſidering that no perſon in England could ſucceed in this attempt, and that we are now able to do it but imperfectly, it is hardly probable that he did it at all.

There ſeems to have been ſome deception in an experiment which the worthy and excellent Dr. Hales communicated

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* Phil. Tranſ. abridged, Vol. x. p. 411.

† Ibid. p 413.

‡ Wilſon's Eſſay, p. 219.

to the Royal Society this year, when he ſays, that he obſerved the electric ſpark from warm iron to be of a bright light colour; from warm copper, green; and from a warm egg, of a yellowiſh flame colour. Theſe experiments, he ſaid, ſeemed to argue, that ſome particles of thoſe different bodies were carried off in the electric flaſhes, whence thoſe different colours were exhibited [*].

I shall conclude this ſection, which might juſtly be intitled the *marvellous,* with mentioning the ſurpriſing effect of an electric ſpark in ſetting fire to a fuſtian frock, on a ſon of Mr. Robert Roche, when he was electrified for ſome diſorder. I do not queſtion the fact. The experiment was repeated, and it anſwered again as well as at the firſt time, when it was merely accidental. The paper containing this account was read at the Royal Society on the 29th of May 1748 [†].

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[*] Phil. Tranſ. abridged, Vol. x. p. 406.

[†] Ibid.

PERIOD IX.

THE EXPERIMENTS AND DISCOVERIES OF DR. FRANKLIN.

SECTION I.

DR. FRANKLIN'S DISCOVERIES CONCERNING THE LEYDEN PHIAL, AND OTHERS CONNECTED WITH THEM.

WE have hitherto ſeen what had been done in electricity by the Engliſh philoſophers, and thoſe on the continent of Europe, till about the year 1750; but our attention is now ſtrongly called to what was doing on the continent of America; where Dr. Franklin and his friends were as aſſiduous in trying experiments, and as ſucceſsful in making diſcoveries, as any of their brethren in Europe. For this purpoſe, we muſt look back a few years. As Dr. Franklin's diſcoveries were made intirely independent of any in Europe, I was unwilling to interrupt the former general account, by introducing them in their proper year. For the ſame reaſon, I imagine, it will be generally more agreeable, to ſee, at one view, what was done in America for ſome conſiderable ſpace of time, without interrupting this account with what was doing, in the mean time, in Europe. I ſhall, therefore, digeſt, in the beſt manner I can, the three firſt publications of Dr. Franklin, entitled *New Experiments and Obſervations on Electricity, made at Philadelphia in America*, communicated in ſeveral letters to Peter Collinſon, Eſq: of London, fellow of the Royal Society; the firſt of which is dated July 28th, 1747, and the laſt April 18th, 1754.

NOTHING was ever written upon the ſubject of electricity which was more generally read and admired in all parts of Europe than theſe letters. There is hardly any European language into which they have not been tranſlated; and, as if this were not ſufficient to make them properly known, a tranſlation of them has lately been made into Latin. It is not eaſy to ſay, whether we are moſt pleaſed with the ſimplicity and perſpicuity with which theſe letters are written, the modeſty with which the author propoſes every hypotheſis of his own, or the noble frankneſs with which he relates his miſtakes, when they were corrected by ſubſequent experiments.

THOUGH the Engliſh have not been backward in acknowledging the great merit of this philoſopher, he has had the ſingular good fortune to be, perhaps, even more celebrated abroad than at home; ſo that to form a juſt idea of the great and deſerved reputation of Dr. Franklin, we muſt read the foreign publications on the ſubject of electricity; in many of which the terms *Frankliniſm*, *Frankliniſt*, and the *Franklinian ſyſtem* occur in almoſt every page. In conſequence of this, Dr. Franklin's principles bid fair to be handed down to poſterity, as expreſſive of the true principles of electricity; juſt as the Newtonian philoſophy is of the true ſyſtem of nature in general.

THE zeal of Dr. Franklin's friends, and his reputation, were conſiderably increaſed by the oppoſition which the Abbé Nollet made to his theory. The Abbé, however, never had any conſiderable ſeconds in the controverſy, and thoſe he had, I am informed, have all deſerted him.

THE riſe of Dr. Franklin's fame in France was firſt occaſioned by a bad tranſlation of his letters falling into the hands of Mr. Buffon, intendant of the French King's gardens, and author of the Natural Hiſtory for which he is famous. This gentleman, having ſucceſsfully repeated Dr. Franklin's experiments, engaged a friend of his, Mr. Dalibard, to reviſe the tranſlation; which was afterwards publiſhed, with a ſhort hiſtory of electricity prefixed to it, and met with a very favourable reception from all ranks of people. What contributed not a little to the ſucceſs of this publication, and to bring Dr. Franklin's principles into vogue in France, was a friend of Mr. Dalibard's exhibiting Dr. Franklin's experiments for money. All the world, in a manner, flocked to ſee theſe new experiments, and all returned full of admiration for the inventor of them *.

DR. FRANKLIN had diſcovered, as well as Dr. Watſon, that the electric matter was not created, but collected by friction, from the neighbouring non-electric bodies. He had obſerved, that it was impoſſible for a man to electrify himſelf, though he ſtood upon glaſs or wax; for that the tube could communicate to him no more electricity than it had received from him in the act of excitation. He had obſerved, that if two perſons ſtood upon wax, one of which rubbed the tube, and the other took the fire from it, they would both appear to be electrified; that if they touched one another after that operation, a ſtronger ſpark would be perceived between them, than when any other perſon touched either of them; and that ſuch a ſpark would take away the electricity of both †.

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* Nollet's Letters, Vol. i. p. 4.

† Franklin's Letters, p. 14.

THESE experiments led the Doctor to think, that the electric fluid was conveyed from the perſon who rubbed the tube to him who touched it; which introduced ſome terms in electricity that had not been uſed before, but have continued in uſe ever ſince. The perſon who touched the tube was ſaid, by Dr. Franklin, to be electrified *poſitively*, or *plus*; being ſuppoſed to receive an additional quantity of electric fire: whereas the perſon who rubbed the tube was ſaid to be electrified *negatively*, or *minus*; being ſuppoſed to have loſt a part of his natural quantity of the electric fluid *.

THIS obſervation was neceſſary to explain the capital diſcovery which Dr. Franklin made with reſpect to the manner of charging the Leyden phial; which is, that when one ſide of the glaſs is electrified poſitively, or *plus*, the other ſide is electrified negatively, or *minus*; ſo that whatever quantity of fire is thrown upon one ſide of the glaſs, the ſame is thrown out of the other; and there is really no more electric fire in the phial after it is charged than before; all that can be done by charging, being to take from one ſide, and convey to the other. Dr. Franklin alſo obſerved, that glaſs was impervious to electricity, and that, therefore, ſince the equilibrium could not be reſtored to the charged phial by any internal communication, it muſt be done by conductors externally, joining the inſide and the outſide †.

THESE capital diſcoveries he made by obſerving, that when a phial was charged, a cork ball ſuſpended on ſilk would be attracted by the outſide coating, when it was repelled by a wire communicating with the inſide; and that it would be repelled by the outſide, when it was attracted by the inſide ‡. But the truth of this maxim appeared more evident when he brought the knob of the wire communicating with the outſide coating within a few inches of the wire communicating with the inſide coating, and ſuſpended a cork ball between them; for, in that caſe, the ball was attracted by them alternately, till the phial was diſcharged[*].

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* Franklin's Letters, p. 15.

† Ibid. p. 3.

‡ Ibid. p. 4.

The European electricians had obſerved, that a phial could not be charged unleſs ſome conductor was in contact with the outſide; but Dr. Franklin made the obſervation more general, and alſo was able, by the principle above mentioned, to give a better account of it. As no more electric fire, he ſays, can be thrown into the inſide of a phial when all is driven from the outſide; ſo, in a phial not yet charged, none can be thrown into the inſide when none can be got from the outſide. He alſo ſhowed, by a beautiful experiment, that, when the phial was charged, one ſide loſt exactly as much as the other gained, in reſtoring the equilibrium. Hanging a ſmall linen thread near the coating of an electrified phial, he obſerved, that every time he brought his finger near the wire, the thread was attracted by the coating. For as the fire was taken from the inſide by touching the wire, the outſide drew in an equal quantity by the thread[†].

He proved that, in diſcharging the phial, the giving from one ſide was exactly equal to the receiving by the other, by placing a perſon upon electrics, and making him diſcharge the phial through his body; when he obſerved, that no electricity remained in him after the diſcharge[‡]. He alſo hung cork balls upon an inſulated conductor at the time of the diſcharge of a phial hanging to it; and obſerved, that if they did not repel before the exploſion, they did not repel at the time, nor after[§]. But the experiment which moſt completely proved, that the coating on one ſide received juſt as much as was emitted from the diſcharge of the other, was the following.

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[*] Franklin's Letters, p. 5.

[†] Ibid.

[‡] Ibid p. 8.

[§] Ibid. p. 84.

HE inſulated his rubber, and then, hanging a phial to his conductor, he found it could not be charged, even though his hand was held conſtantly to it; becauſe, though the electric fire might leave the outſide of the phial, there was none collected by the rubber to be conveyed into the inſide. He then took away his hand from the phial, and forming a communication, by a wire from the outſide coating to the inſulated rubber, he found that it was charged with eaſe. In this caſe, it was plain, that the very ſame fire which left the outſide coating was conveyed by the way of the rubber, the globe, the conductor, and the wire of the phial, into the inſide *.

DR. FRANKLIN's new theory of charging the Leyden phial led him to obſerve a greater variety of facts, relating both to charging and diſcharging it, than other philoſophers had attended to. He found that the phial would be electrified as ſtrongly if it were held by the hook, and the coating applied to the globe or tube, as if it were held by the coating, and the hook applied; and, conſequently, that there would be the ſame exploſion, and ſhock, if the electrified phial were held in one hand by the hook, and the coating touched with the other, as when held by the coating, and touched at the hook. To take the charged phial by the hook with ſafety, and not diminiſh its force, he obſerves, that it muſt firſt be ſet down upon electrics per ſe †.

DR. FRANKLIN obſerved, that if a man held in his hand two phials, the one fully electrified, and the other not at all, and brought their hooks together, he would have but half a ſhock: for the phials would both remain only half elec-

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* Franklin's Letters, p. 83.

† Ibid. p. 19.

trified, the one being half charged, and the other half diſcharged *.

If two phials were charged both through their hooks, a cork ball ſuſpended on ſilk, and hanging between them, would firſt be attracted, and then repelled by both; but if they were electrified, the one through the wire, and the other through the coating, the ball would play vigorouſly between them both, till they were nearly diſcharged †. The Doctor did not, at that time, take notice, that if the phials were both charged through their coatings (by which both the hooks would have been electrified minus) the ball would be repelled by them both, as when they were electrified plus. And when he, afterwards, obſerved that two bodies electrified minus repelled one another, he ſeems to have been ſurpriſed at the appearance, and acknowledged that he could not ſatisfactorily account for it ‡.

It was known to every electrician, that a globe or tube wet on the inſide would afford little or no fire; but no good reaſon was given for it, before Dr. Franklin attempted its explanation by the help of his general maxim. He ſays, that when a tube lined with any non-electric is rubbed, what is collected from the hand by the downward ſtroke enters the pores of the glaſs, driving an equal quantity out of the inner ſurface, into the non-electric lining; and that the hand, in paſſing up to take a ſecond ſtroke, takes out again what had been thrown into the outward ſurface, the inner ſurface at the ſame time receiving back again what it had given to the non-electric lining; ſo that the particles of the electric fluid go in and out of their pores, upon every ſtroke given to the tube §.

If, in theſe circumſtances, a wire was put into the tube, he obſerved, that, if one perſon touched the wire, while another

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* Franklin's Letters, p. 21. † Ibid. ‡ Ibid. p. 34. § Ibid. p. 76.

was rubbing the tube, and took care to withdraw his finger as ſoon as he had taken the ſpark, which had been made to fly from the inſide, it would be charged *.

IF the tube was exhauſted of air, he obſerves, that a non-electric lining in contact with the wire was not neceſſary; for that, in vacuo, the electric fire would fly freely from the inner ſurface, without a non-electric conductor †.

UPON the ſame principle he accounts for the effects of an excited electric being perceived through the glaſs in the vacuum beyond it. The tube and its excited atmoſphere, being brought near a glaſs veſſel, repels the electric fluid from the inner ſurface of the glaſs; and this fluid, iſſuing from the inner ſurface acts upon light bodies in the vacuum, both in its paſſage from the glaſs, and likewiſe in its return to it, when the excited electric on the outſide is withdrawn ‡.

THIS maxim, that whatever the phial takes in at one ſurface it loſes at the other, led Dr. Franklin to think of charging ſeveral phials together with the ſame trouble, by connecting the outſide of one with the inſide of another; whereby the fluid that was driven out of the firſt would be received by the ſecond, and what was driven out of the ſecond would be received by the third, &c. By this means he found, that a great number of bottles might be charged with the ſame labour as one only; and that they might be charged equally high, were it not that every bottle receives the new fire, and loſes its old with ſome reluctance, or rather gives ſome ſmall reſiſtance to the charging. This cirumſtance, he ſays, in a number of bottles, becomes more equal to the charging power, and ſo repels the fire back again on the globe ſooner than a ſingle bottle would do §.

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* Franklin's Letters, p. 77. † Ibid. ‡ Ibid. p. 78. § Ibid. p. 12.

Upon this principle Dr. Franklin conſtructed an *electrical battery*, conſiſting of eleven panes of large ſaſh glaſs, coated on each ſide, and ſo connected, that charging one of them would charge them all. Then having a contrivance to bring all the giving ſides in contact with one wire, and all the receiving ſides with another, he united the force of all the plates, and diſcharged them all at once *.

When Dr. Franklin firſt began his experiments upon the Leyden phial, he imagined that the electric fire was all crowded into the ſubſtance of the non-electric in contact with the glaſs; but he afterwards found, that its power of giving a ſhock lay in the glaſs itſelf, and not in the coating, by the following ingenious analyſis of the bottle.

In order to find where the ſtrength of the charged bottle lay, he placed it upon glaſs; then firſt took out the cork and the wire, and finding the virtue was not in them; he touched the outſide coating with one hand, and put the finger of the other into the mouth of the bottle; when the ſhock was felt quite as ſtrong as if the cork and wire had been in it. He then charged the phial again, and pouring out the water into an empty bottle inſulated, expected that if the force reſided in the water it would give the ſhock, but he found it gave none. He then judged that the electric fire muſt either have been loſt in decanting, or muſt remain in the bottle; and the latter he found to be true; for, filling the charged bottle with freſh water, he found the ſhock, and was ſatisfied that the power of giving it reſided in the glaſs itſelf †.

The Doctor made the ſame experiment with panes of glaſs, laying the coating on lightly, and changing it, as he had before changed the water in the bottle, and the reſult was the ſame in both *. This experiment is more ſatisfactory than the former; becauſe, when the water is poured out of the phial, there ſtill remains a thin coating of the fluid, which might be thought to contain the power of giving a ſhock.

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* Franklin's Letters, p. 26.

† Ibid. p. 24.

THAT the electric fire reſided in the glaſs, was alſo farther evident from this conſideration, that when glaſs was gilt, the diſcharging of it would make a round hole, tearing off a part of the gilding, which, the Doctor thought, could only have been done by the fire coming out of the glaſs through the gilding. He alſo ſays, that when the gilding was varniſhed even with turpentine, this varniſh, though dry and hard, would be burned by the ſpark driven through it, yielding a ſtrong ſmell, and a viſible ſmoke. Alſo, that when a ſpark was driven through paper, it would be blackened by the ſmoke, which ſometimes penetrated ſeveral of the leaves, and that part of the gilding which had been torn off was found forcibly driven into the hole made in the paper by the ſtroke. He alſo obſerved, that when a thin bottle was broken by a charge, the glaſs was broken inwards, at the ſame time that the gilding was broken outwards †.

LASTLY, Dr. Franklin diſcovered, that ſeveral ſubſtances which would conduct electricity in general, would not conduct the ſhock of a charged phial. A wet packthread, for inſtance, though it tranſmitted electricity very well, ſometimes failed to conduct a ſhock; as alſo did a cake of ice. Dry earth too rammed into a glaſs tube intirely failed to conduct a ſhock, and indeed would convey electricity but very imperfectly ‡.

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* Franklin's Letters p. 25.

† Ibid. p 32.

‡ Ibid. p. 33.

SECTION

SECTION II.

Dr. FRANKLIN's discoveries concerning the similarity of lightning and electricity.

THE greateſt diſcovery which Dr. Franklin made concerning electricity, and which has been of the greateſt practical uſe to mankind, was that of the perfect ſimilarity between electricity and lightning. The analogy between theſe two powers had not been wholly unobſerved by philoſophers, and eſpecially by electricians, before the publication of Dr. Franklin's diſcovery. It was ſo obvious, that it had ſtruck ſeveral perſons. I ſhall give one inſtance, in the ſagacious Abbé Nollet.

The Abbé ſays*, "If any one ſhould take upon him to "prove, from a well connected compariſon of phenomena, "that thunder is, in the hands of nature, what electricity is in "ours, that the wonders which we now exhibit at our plea- "ſure are little imitations of thoſe great effects which frighten us, "and that the whole depends upon the ſame mechaniſm; if it "is to be demonſtrated, that a cloud, prepared by the action of "the winds, by heat, by a mixture of exhalations, &c. is op- "poſite to a terreſtrial object; that this is the electrized body, "and, at a certain proximity from that which is not; I avow "that this idea, if it was well ſupported, would give me a

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* Leçons de Phyſique, Vol. iv. p. 34.

" great deal of pleaſure; and in ſupport of it, how many ſpe-
" cious reaſons preſent themſelves to a man who is well acquaint-
" ed with electricity. The univerſality of the electric matter, the
" readineſs of its action, its inflammability, and its activity in
" giving fire to other bodies, its property of ſtriking bodies
" externally and internally, even to their ſmalleſt parts, the re-
" markable example we have of this effect in the experiment
" of Leyden, the idea which we might truly adopt in ſup-
" poſing a greater degree of electric power, &c. all theſe
" points of analogy, which I have been ſome time meditating,
" begin to make me believe, that one might, by taking elec-
" tricity for the model, form to one's ſelf, in relation to thunder
" and lightning, more perfect and more probable ideas than
" what have been offered hitherto, &c."

MR. WINCKLER alſo enumerated many particulars, in which electricity and lightning reſemble one another *.

BUT though the Abbé, and others, had been ſtruck with the obvious analogy between lightning and electricity, they went no farther than theſe arguments *a priori*. It was Dr. Franklin who firſt propoſed a method of verifying this hypotheſis, entertaining the bold thought, as the Abbé Nollet expreſſes it, of bringing lightning from the heavens, of thinking that pointed iron rods, fixed in the air, when the atmoſphere was loaded with lightning, might draw from it the matter of the thunderbolt, and diſcharge it without noiſe or danger into the immenſe body of the earth, where it would remain as it were abſorbed.

MOREOVER, though Dr. Franklin's directions were firſt begun to be put in execution in France, he himſelf completed the demonſtration of his own problem, before he heard of what had been done elſewhere: and he extended his experiments ſo far as actually to imitate almoſt all the known effects of lightning by electricity, and to perform every electrical experiment by lightning.

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* Dantzick Memoirs, Vol. iii. p. 528.

But before I relate any of Dr. Franklin's experiments concerning lightning, I muſt take notice of what he obſerved concerning the power of *pointed bodies*, by means of which he was enabled to carry his great deſigns into execution. For he was properly the firſt who obſerved the intire and wonderful effect of pointed bodies, both in drawing, and throwing off the electric fire.

It was a ſmall ſtep towards diſcovering the effect of pointed bodies, that Carolus Auguſtus van Bergen, profeſſor of medicine at Frankfort on the Oder, obſerved that ſparks taken from a poliſhed body were ſtronger than thoſe from a rough one. He could fire ſpirit eaſily with a poliſhed conductor, but with difficulty by means of one not poliſhed *.

Mr. Jallabert was perhaps the firſt who obſerved that a body, pointed at one end, and round at another, produced different appearances upon the ſame body, according as the pointed, or round end was preſented to it. But as Mr. Nollet, in whoſe preſence he made the experiment, ſays, the effect was not conſtant, and nothing was inferred from it †. And the Abbé acknowledges, that Dr. Franklin was the firſt who ſhowed the property of pointed bodies, in drawing off electricity more effectually, and at greater diſtances than other bodies could do it ‡.

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* Dantzick Memoirs, Vol. ii. p. 378.

† Lettres, Vol. i. p. 130.

‡ Recherches, p. 132. Dr. Franklin, in the new edition of his Letters, p. 5, ſays, that the power of points to throw off the electric fire, was communicated to him by his friend Mr. Thomas Hopkinſon, who electrified an iron ball of three or four inches diameter, with a needle faſtened to it, expecting to draw a ſtronger ſpark from the point, as from a kind of focus, but was ſurpriſed to find little or none.

HE electrified an iron ſhot, three or four inches in diameter, and obſerved, that it would not attract a thread, when the point of a needle was preſented to it; but that this was not the caſe, unleſs the pointed body had a communication with the earth; for, preſenting the ſame pointed body, ſtuck on a piece of ſealing-wax, it had not that effect; though the moment the pointed body was touched with his finger, the electricity of the ball to which it was ſuſpended was diſcharged. The converſe of this he proved, by finding it impoſſible to electrify the iron ſhot when a ſharp needle lay upon it*.

BY obſerving points of different degrees of acuteneſs, Dr. Franklin corrected the concluſion of Mr. Ellicott, and other Engliſh electricians, that a pointed body, as a piece of leaf gold, would always be ſuſpended nearer to the plate which was unelectrified than that which was electrified, if it were put between them. For the Doctor obſerved, that it always removed fartheſt from that plate to which its ſharpeſt point was preſented, whether it was electrified or not; and if one of the points was very blunt, and the other very ſharp, it would be ſuſpended in the air by its blunt end, near the electrified body, without any unelectrified plate being held below it at all †.

DR. FRANKLIN endeavoured to account for this effect of pointed bodies, by ſuppoſing that the baſe on which the electric fluid at the point of an electrified body reſted, being ſmall, the attraction by which the fluid was held to the body was ſlight; and that, for the ſame reaſon, the reſiſtance to the entrance of the fluid was proportionably weaker in that place than where the ſurface was flat ‡. But he himſelf candidly owns, that he was not quite ſatisfied with this hypotheſis. Whatever we think of Dr. Franklin's theory of the influence of pointed conductors in drawing and throwing off the electric fluid, the world is greatly indebted to him for the practical uſe he made of this doctrine *.

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* Franklin's Letters, p. 56, &c.

† Ibid. p. 67.

‡ Ibid. p. 56.

THE manner in which Dr. Franklin firſt conceived the practicability of drawing lightning from the clouds may be ſeen in an extract which he has given us from his memorandums, November 7th, 1749. After enumerating all the known points of reſemblance between lightning and electricity, he concludes with ſaying, " The electric fluid is attracted by points. We do " not know whether this property be in lightning, but ſince they " agree in all the particulars in which we can already compare " them, it is not probable that they agree likewiſe in this. Let the " experiment be made." Every circumſtance relating to a diſcovery of ſo much importance as this, is intereſting and pleaſing †.

DR. FRANKLIN begins his account of the ſimilarity of the electric fluid and lightning by cautioning his readers not to be ſtaggered at the great difference of the effects in point of degree; ſince that is no argument of any diſparity in their nature. It is no wonder, ſays he, if the effects of the one ſhould be ſo much greater than thoſe of the other. For if two gun barrels electrified will ſtrike at two inches diſtance, and make a loud report, at how great a diſtance will ten thouſand acres of electrified cloud ſtrike, and give its fire, and how loud muſt be that crack ‡ !

I SHALL digeſt all Dr. Franklin's obſervations concerning lightning under the ſeveral points of reſemblance which he obſerved between it and electricity, mentioning theſe points of ſimilarity in the order in which he himſelf remarked them; only bringing into one place the obſervations which may happen to lie in different parts of his letters, when they relate to the ſame ſubject.

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* Franklin's Letters, p. 62.

† Letters, New Edit. p. 323.

‡ Franklin's Letters, p. 44.

1. FLASHES of lightning, he begins with obſerving, are generally ſeen crooked, and waving in the air. The ſame, ſays he, is the electric ſpark always, when it is drawn from an irregular body at ſome diſtance *. He might have added, when it is drawn by an irregular body, or through a ſpace in which the beſt conductors are diſpoſed in an irregular manner, which is always the caſe in the heterogeneous atmoſphere of our globe.

2. LIGHTNING ſtrikes the higheſt and moſt pointed objects in its way preferably to others, as high hills, and trees, towers, ſpires, maſts of ſhips, points of ſpears, &c. In like manner, all pointed conductors receive or throw off the electric fluid more readily than thoſe which are terminated by flat ſurfaces †.

3. LIGHTNING is obſerved to take the readieſt and beſt conductor. So does electricity in the diſcharge of the Leyden phial. For this reaſon, the Doctor ſuppoſes that it would be ſafer, during a thunder ſtorm, to have one's cloaths wet than dry, as the lightning might then, in a great meaſure, be tranſmitted to the ground, by the water, on the outſide of the body. It is found, ſays he, that a wet rat cannot be killed by the exploſion of the electrical bottle, but that a dry rat may †.

4. LIGHTNING burns. So does electricity. Dr. Franklin ſays, that he could kindle with it hard dry roſin, ſpirits unwarmed, and even wood. He ſays, that he fired gunpowder, by only ramming it hard in a cartridge, into each end of which pointed wires were introduced, and brought within half an inch of one another, and diſcharging a ſhock through them ‡.

5. LIGHTNING ſometimes diſſolves metals. So does electricity, though the Doctor was miſtaken when he imagined it was by a cold fuſion, as will appear in its proper place. The method

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* Franklin's Letters, p. 46. † Ibid. p. 47. ‡ Ibid. p. 48. 92.

in which Dr. Franklin made electricity melt metals was by putting thin pieces of them between two panes of glaſs bound faſt together, and ſending an electric ſhock through them. Sometimes the piece of glaſs, by which they were confined, would be ſhattered to pieces by the diſcharge, and be broken into a kind of coarſe ſand, which once happened with pieces of thick looking-glaſs; but if they remained whole, the piece of metal would be miſſing in ſeveral places where it had lain between them, and inſtead of it, a metallic ſtain would be ſeen on both the glaſſes, the ſtains on the under and upper glaſs being exactly ſimilar in the minuteſt ſtroke *.

A piece of leaf gold treated in this manner appeared not only to have been melted, but, as the Doctor thought, even vitrified, or otherwiſe ſo driven into the pores of the glaſs, as to be protected by it from the action of the ſtrongeſt aqua regis. Sometimes he obſerved that the metallic ſtains would ſpread a little wider than the breadth of the thin pieces of metal. True gold, he obſerved, made a darker ſtain, ſomewhat reddiſh, and ſilver a greeniſh ſtain †.

Mr. Wilson ſuppoſes that, in this experiment, the gold was not driven into the pores of the glaſs, but only into ſo near a contact with the ſurface of the glaſs, as to be held there by an exceedingly great force; ſuch an one, he ſays, as is exerted at the ſurface of all bodies whatever ‡.

6. Lightning rends ſome bodies. The ſame does electricity §. The Doctor obſerves, that the electric ſpark would ſtrike a hole through a quire of paper. When wood, bricks, ſtone, &c. are rent by lightning, he takes notice, that the ſplinters will fly off on that ſide where there is the leaſt reſiſtance. In like manner, he ſays, when a hole is ſtruck through a piece

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* Franklin's Letters, p. 48. 65.

† Ibid. p. 68.

‡ Hoadley and Wilſon, p. 68.

§ Franklin's Letters, p. 49.

of paſteboard by an electrified jar, if the ſurfaces of the paſteboard are not confined and compreſſed, there will be a bur raiſed all round the hole on both ſides of the paſteboard ; but that if one ſide be confined, ſo that the bur cannot be raiſed on that ſide, it will all be raiſed on the other ſide, which way ſoever the fluid was directed. For the bur round the outſide of the hole is the effect of the exploſion, which is made every way from the center of the electric ſtream, and not an effect of its direction *.

7. LIGHTNING has often been known to ſtrike people blind. And a pigeon, after a violent ſhock of electricity, by which the Doctor intended to have killed it, was obſerved to have been ſtruck blind likewiſe †.

8. IN a thunder ſtorm at Stretham, deſcribed by Dr. Miles ‡, the lightning ſtripped off ſome paint which had covered a gilded molding of a pannel of wainſcot, without hurting the reſt of the paint. Dr. Franklin imitated this, by paſting a ſlip of paper over the filleting of gold on the cover of a book, and ſending an electric flaſh through it. The paper was torn off from end to end, with ſuch force, that it was broken in ſeveral places ; and in others there was brought away part of the grain of the Turkey leather in which the book was bound. This convinced the Doctor, that if it had been paint, it would have been ſtripped off in the ſame manner with that on the wainſcot at Stretham §.

9. LIGHTNING deſtroys animal life. Animals have likewiſe been killed by the ſhock of electricity. The largeſt animals which Dr. Franklin and his friends had been able to kill were a hen, and a turkey which weighed about ten pounds ||.

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* Franklin's Letters, p. 124.

† Ibid. p. 36.

‡ Phil. Tranſ. abridged, Vol. xlv. p. 387.

§ Ibid. p. 64.

|| Franklin's Letters, p. 86. 153.

10. Magnets have been obſerved to loſe their virtue, or to have their poles reverſed by lightning. The ſame did Dr. Franklin by electricity. By electricity he frequently gave polarity to needles, and reverſed them at pleaſure. A ſhock from four large jars, ſent through a fine ſewing needle, he ſays, gave it polarity, ſo that it would traverſe when laid on water. What is moſt remarkable in theſe electrical experiments upon magnets is, that if the needle, when it was ſtruck, lay Eaſt and Weſt, the end which was entered by the electric blaſt pointed North, but that if it lay North and South, the end which lay towards the North, would continue to point North, whether the fire entered at that end or the contrary; though he imagined, that a ſtronger ſtroke would have reverſed the poles even in that ſituation, an effect which had been known to have been produced by lightning. He alſo obſerved, that the polarity was ſtrongeſt when the needle was ſtruck lying North and South, and weakeſt when it lay Eaſt and Weſt. He takes notice that, in theſe experiments, the needle, in ſome caſes, would be finely blued, like the ſpring of a watch, by the electric flame; in which caſe the colour given by a flaſh from two jars only might be wiped off, but that a flaſh from four jars fixed it, and frequently melted the needles. The jars which the Doctor uſed held ſeven or eight gallons, and were coated and lined with tinfoil *.

To demonſtrate, in the completeſt manner poſſible, the ſameneſs of the electric fluid with the matter of lightning, Dr. Franklin, aſtoniſhing as it muſt have appeared, contrived actually to bring lightning from the heavens, by means of an electrical kite, which he raiſed when a ſtorm of thunder was perceived to be coming on. This kite had a pointed wire fixed upon it, by which it drew the lightning from the clouds. This light-

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* Franklin's Letters, p. 90, &c.

ning deſcended by the hempen ſtring, and was received by a key tied to the extremity of it; that part of the ſtring which was held in his hand being of ſilk, that the electric virtue might ſtop when it came to the key. He found that the ſtring would conduct electricity even when nearly dry, but that when it was wet, it would conduct it quite freely; ſo that it would ſtream out plentifully from the key, at the approach of a perſon's finger *.

AT this key he charged phials, and from electric fire thus obtained, he kindled ſpirits, and performed all other electrical experiments which are uſually exhibited by an excited globe or tube.

As every circumſtance relating to ſo capital a diſcovery as this (the greateſt, perhaps, that has been made in the whole compaſs of philoſophy, ſince the time of Sir Iſaac Newton) cannot but give pleaſure to all my readers, I ſhall endeavour to gratify them with the communication of a few particulars which I have from the beſt authority.

THE Doctor, after having publiſhed his method of verifying his hypotheſis concerning the ſameneſs of electricity with the matter of lightning, was waiting for the erection of a ſpire in Philadelphia to carry his views into execution; not imagining that a pointed rod, of a moderate height, could anſwer the purpoſe; when it occurred to him, that, by means of a common kite, he could have a readier and better acceſs to the regions of thunder than by any ſpire whatever. Preparing, therefore, a large ſilk handkerchief, and two croſs ſticks, of a proper length, on which to extend it, he took the opportunity of the firſt approaching thunder ſtorm to take a walk into a field, in which there was a ſhed convenient for his purpoſe. But dreading the ridicule which too commonly attends unſucceſsful attempts in ſcience, he

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* Franklin's Letters, p. 106, &c.

communicated his intended experiment to no body but his ſon, who aſſiſted him in raiſing the kite.

THE kite being raiſed, a conſiderable time elapſed before there was any appearance of its being electrified. One very promiſing cloud had paſſed over it without any effect; when, at length, juſt as he was beginning to deſpair of his contrivance, he obſerved ſome loſe threads of the hempen ſtring to ſtand erect, and to avoid one another, juſt as if they had been ſuſpended on a common conductor. Struck with this promiſing appearance, he immediately preſented his knucle to the key, and (let the reader judge of the exquiſite pleaſure he muſt have felt at that moment) the diſcovery was complete. He perceived a very evident electric ſpark. Others ſucceeded, even before the ſtring was wet, ſo as to put the matter paſt all diſpute, and when the rain had wetted the ſtring, he collected electric fire very copiouſly. This happened in June 1752, a month after the electricians in France had verified the ſame theory, but before he had heard of any thing that they had done.

BESIDES this kite, Dr. Franklin had afterwards an inſulated iron rod to draw the lightning into his houſe, in order to make experiments whenever there ſhould be a conſiderable quantity of it in the atmoſphere; and that he might not loſe any opportunity of that nature, he connected two bells with this apparatus, which gave him notice, by their ringing, whenever his rod was electrified *.

THE Doctor being able, in this manner, to draw the lightning into his houſe, and make experiments with it at his leiſure; and being certain that it was in all reſpects of the ſame nature with electricity, he was deſirous to know if it was of the poſitive or negative kind. The firſt time he ſucceeded in making an ex-

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* Franklin's Letters, p. 112.

periment for this purpoſe was the 12th of April 1753, when it appeared that the lightning was negative. Having found that the clouds electrified negatively in eight ſucceſſive thunder guſts, he concluded they were always electrified negatively, and formed a theory to account for it. But he afterwards found he had concluded too ſoon. For, on the ſixth of June following, he met with one cloud which was electrified poſitively; upon which he corrected his former theory, but did not ſeem able perfectly to ſatisfy himſelf with any other. The Doctor ſometimes found the clouds would change from poſitive to negative electricity ſeveral times in the courſe of one thunder guſt, and he once obſerved the air to be ſtrongly electrified during a fall of ſnow, when there was no thunder at all *.

But the grand practical uſe which Dr. Franklin made of his diſcovery of the ſameneſs of electricity and lightning, was to ſecure buildings from being damaged by lightning, a thing of vaſt conſequence in all parts of the world, but more eſpecially in ſeveral parts of North America, where thunder ſtorms are more frequent, and their effects, in that dry air, more dreadful, than they are ever known to be with us.

This great end Dr. Franklin accompliſhed by ſo eaſy a method, and by ſo cheap, and ſeemingly trifling apparatus, as fixing a pointed metalline rod higher than any part of the building, and communicating with the ground, or rather the neareſt water. This wire the lightning was ſure to ſeize upon, preferably to any other part of the building; whereby this dangerous power would be ſafely conducted to the earth, and diſſipated, without doing any harm to the building †.

Dr. Franklin was of opinion, that a wire of a quarter of an inch in thickneſs would be ſufficient to conduct a greater

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[*] Franklin's Letters, p. 112, &c.

[†] Ibid. p. 62. 124.

quantity of lightning than was ever actually diſcharged from the clouds in one ſtroke. He found, that the gilding of a book was ſufficient to conduct the charge of five large jars, and thought that it would probably have conducted the charge of many more. He alſo found by experiment, that if a wire was deſtroyed by an exploſion, it was yet ſufficient to conduct that particular ſtroke, though it was thereby rendered incapable of conducting another *.

The Doctor alſo ſuppoſed, that pointed rods erected on edifices might likewiſe often prevent a ſtroke of lightning in the following manner. He ſays, that an eye ſo ſituated as to view horizontally the underſide of a thunder cloud, will ſee it very ragged, with a number of ſeparate fragments, or petty clouds, one under another, the loweſt ſometimes not far from the earth. Theſe, as ſo many ſtepping ſtones, aſſiſt in conducting a ſtroke between a cloud and a building. To repreſent theſe by an experiment, he directs us to take two or three locks of fine looſe cotton and connect one of them with the prime conductor, by a fine thread of two inches (which may be ſpun out of the ſame lock) another to that, and a third to the ſecond, by like threads. He then bids us to turn the globe, and ſays we ſhall ſee theſe locks extending themſelves towards the table (as the lower ſmall clouds do towards the earth) but, that, on preſenting a ſharp point, erect under the loweſt, it will ſhrink up to the ſecond, the ſecond to the firſt, and all together to the prime conductor, where they will continue as long as the point continues under them. A moſt ingenious and beautiful experiment! May not, he adds, in like manner, the ſmall electrified clouds, whoſe equilibrium with the earth is ſoon reſtored by the point, riſe up to the main body, and by that means occaſion ſo large a vacancy, as that the grand cloud cannot ſtrike in that place †.

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* Franklin's Letters, p. 124, 125.

† Ibid. p. 121, &c.

MR. WILCKE, in his remarks on Dr. Franklin's letters, ſays, that on the 20th of Auguſt 1758, he ſaw this ſuppoſition verified; as he was viewing a large fringed cloud, ſtrongly electrified, paſſing over a foreſt of tall fir trees. The ragged and depending parts of the large cloud were firſt attracted lower, and then ſuddenly roſe higher, and joined the large cloud *.

HE was alſo an eye witneſs of two clouds lying one over the other, approaching, and flaſhing into one another. The lightning ſpread itſelf over all the parts of the blacker cloud, which was negative, and which immediately began to diſſolve in rain †.

DR. FRANKLIN adviſes perſons who are apprehenſive of danger from lightning, to ſit in the middle of a room (provided it be not under a metal luſtre ſuſpended by a chain) ſitting on one chair, and laying their feet on another. It is ſtill ſafer, he ſays, to bring two or three mattreſſes, or beds, into the middle of the room, and folding them double, to place the chairs upon them, for they not being ſo good conductors as the walls, the lightning will not chuſe to paſs through them; but the ſafeſt place of all is in a hammock hung in ſilken cords, at an equal diſtance from all the ſides of the room, p. 484. I would add, that the place of moſt abſolute ſafety muſt be the cellar, and eſpecially the middle of it; for when a perſon is lower than the ſurface of the earth, the lightning muſt ſtrike it before it can poſſibly reach him. In the fields, the place of ſafety is, within a few yards of a tree, but not quite near it.

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* Wilcke's Tranſlation, p. 351.

† Franklin's Letters, p. 259.

SECTION III.

Miscellaneous discoveries of Dr. FRANKLIN, and his friends in America, during the same Period.

DR. FRANKLIN, retaining the common opinion, that electrified bodies have real atmoſpheres of the electric fluid (conſiſting of particles at ſome diſtance from the ſurface of the body, but always going along with it) obſerved that theſe atmoſpheres and the air did not ſeem to exclude one another; though, he ſays, this be difficult to conceive, conſidering that they are generally ſuppoſed to repel one another.

An electric atmoſphere, he ſays, raiſed round a thick wire, inſerted into a phial, drives out none of the air it contained; nor on withdrawing that atmoſphere, will any air ruſh in, as he found by a very curious experiment, accurately made; whence he alſo concluded, that the elaſticity of the air was not affected by it[*].

The experiment, as the Doctor informs me, was made with a ſmall glaſs ſyphon, one leg paſſing through the cork into the bottle. The other leg had in it a drop of red ink, which readily moved on the leaſt change of heat or cold in the air contained in the phial; but not at all on the air's being electrified.

He alſo made an experiment which would ſeem to prove the immobility, as we may ſay, of theſe atmoſpheres by any external force, if they have any exiſtence at all; but others may think it is rather an argument againſt their exiſtence. He electrified a large cork ball faſtened to the end of a ſilk ſtring three feet long; and, taking the other end in his hand, he whirled it round, like a ſling, a hundred times in the open air, with the ſwifteſt motion he could poſſibly give it; and obſerved, that it ſtill retained its electric atmoſphere, though it muſt have paſſed through eight hundred yards of air *.

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[*] Franklin's Letters, p. 98.

To ſhow that a body, in different circumſtances of dilatation and contraction, is capable of receiving, or retaining more or leſs of the electric fluid on its ſurface, he made the following curious experiment. He electrified a ſilver cann, in which there were about three yards of braſs chain, one end of which he could raiſe to what height he pleaſed, by means of a pully and a ſilken cord. He ſuſpended a lock of cotton by a ſilken ſtring from the ceiling of the room, making it hang near the cup; and obſerved, that every time he drew up the chain, the cotton approached nearer to the cup, and as conſtantly receded from it when the chain was let down. From this experiment it was evident, he ſays, that the atmoſphere about the cup was diminiſhed by raiſing the chain, and increaſed by lowering it; and that the atmoſphere of the chain muſt have been drawn from that of the cup when it was raiſed, and have returned to it again when it was let down †.

To make electric atmoſpheres in ſome meaſure viſible, the Doctor uſed to drop roſin on hot iron plates held under bodies electrified; and, in a ſtill room, the ſmoke would aſcend, and form viſible atmoſpheres round the bodies, making them look very beautiful. In trying in what circumſtances, the repellency between an electrified iron ball, and a ſmall cork ball would be altered, he

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* Franklin's Letters, p. 97.

† Ibid. p. 121.

obſerved, that the ſmoke of roſin did not deſtroy their repellency, but was attracted both by the iron and the cork *.

The Doctor obſerved, that ſilver expoſed to the electric ſpark would acquire a blue ſtain, and that iron would ſeem corroded by it; but he could never perceive any impreſſion made on gold, braſs, or tin. The ſpots on the ſilver or iron were always the ſame, whether they received the ſpark from lead, braſs, gold, or ſilver; and the ſmell of the electric fire was the ſame, through whatever bodies it was conveyed †.

While we are attending to what was done by Dr. Franklin at Philadelphia, we muſt by no means overlook what was done by Mr. Kinnerſley, the Doctor's friend, while at Boſton in New England. Some of his obſervations, of which an account is given in the Doctor's letters, are very curious; and ſome later accounts, which he himſelf has tranſmitted to England, ſeem to promiſe, that, if he continue his electrical inquiries, his name, after that of his friend, will be ſecond to few in the hiſtory of electricity.

He firſt diſtinguiſhed himſelf by re-diſcovering Mr. Du Fay's two contrary electricities of glaſs and ſulphur, with which both he and Dr. Franklin were at that time wholly unacquainted. But Mr. Kinnerſley had a great advantage over Mr. Du Fay; for making his experiments in a more advanced ſtate of the ſcience, he ſaw immediately, that the two contrary electricities of glaſs and ſulphur were the very ſame poſitive and negative electricities, which had juſt been diſcovered by Dr. Watſon and Dr. Franklin.

He obſerved, that a cork ball, electrified by a conductor from excited glaſs, would be attracted by excited amber and ſulphur, and repelled by excited glaſs and china; that electrifying the ball with the wire of a charged phial, it would be repelled by excited

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* Franklin's Letters, p. 55.

† Ibid. p. 81, 98.

glaſs, but attracted by excited ſulphur; and that when he electrified it by ſulphur or amber, till it became repelled by them, it would be attracted by the wire of the phial, and repelled by its coating. Theſe experiments ſurpriſed him very much, but by analogy he was led to infer, *a priori*, the following paradoxes, as he calls them, which were afterwards verified by Dr. Franklin at his requeſt *.

" 1. If a glaſs globe be placed at one end of a prime con-
" ductor, and a ſulphur one at the other, both being equally
" in good order, and in equal motion, not a ſpark of fire can
" be obtained from the conductor, but one globe will draw out
" as faſt as the other gives in.

" 2. If a phial be ſuſpended on the conductor with a chain
" from its coating to the table, and only one of the globes be
" made uſe of at a time, twenty turns of the wheel, for inſtance,
" will charge it; after which, as many turns of the other wheel
" will diſcharge it; and as many more will charge it again.

" 3. The globes being both in motion, each having a ſe-
" parate conductor, with a phial ſuſpended on one of them,
" and the chain faſtened to the other; the phial will become
" charged, one globe charging poſitively, and the other nega-
" tively.

" 4. The phial being thus charged, hang it in like manner,
" on the other conductor. Set both wheels a-going again,
" and the ſame number of turns that charged it before will
" now diſcharge it, and the ſame number repeated will charge
" it again.

" 5. When each globe communicates with the ſame prime
" conductor, having a chain hanging from it to the table, one
" of them, when in motion (but which I cannot ſay) will draw

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* Franklin's Letters, p. 99.

"fire up through the cuſhion, and diſcharge it, through the "chain; and the other will draw it up through the chain, and "diſcharge it through the cuſhion*."

WHEN Mr. Kinnerſley was adviſing his friend to try the experiments with the ſulphur globe, he cautions him not to make uſe of chalk on the cuſhion, telling him that ſome fine powdered ſulphur would do better. And he expreſſes his hope that if the Doctor ſhould find the two globes to charge the prime conductor differently, he would be able to diſcover ſome method of determining which it was that charged poſitively.

DR. FRANKLIN, when theſe experiments and conjectures were propoſed to him, had no idea of their having any real foundation; but imagined, that the different attractions and repulſions obſerved by Mr. Kinnerſley proceeded rather from the greater or ſmaller quantities of the electric fire, obtained from different bodies, than from its being either of a different kind, or having a different direction. But finding, upon trial, that the principal of Mr. Kinnerſley's ſuppoſitions were verified by fact, he had no doubt of the reſt †.

IN anſwer to the doubt of Mr. Kinnerſley, whether the glaſs, or the ſulphur electrified poſitively, the Doctor gave it as his opinion, that the glaſs globe charged poſitively, and the ſulphur negatively, for the following reaſons.

1. BECAUSE, though the ſulphur globe ſeemed to work equally well with the glaſs one, yet it could never occaſion ſo large, and ſo diſtant a ſpark between his finger and conductor as when the glaſs globe was uſed. But what he adds to confirm this proof does not ſeem to be ſatisfactory. He ſuppoſes that bodies of a certain bigneſs cannot ſo eaſily part with the quantity of electric fluid which they have, and hold attracted with-

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* Franklin's Letters, p. 100.

† Ibid. p. 102, 103.

in their ſubſtance, as they can receive an additional quantity upon their ſurface, by way of atmoſphere; and that therefore ſo much could not be drawn out of the conductor, as might be thrown on it *.

2. He obſerved that the ſtream or bruſh of fire, appearing at the end of the wire connected with the conductor, was long, large, and much diverging when the glaſs globe was uſed, and made a ſnapping or rattling noiſe; but that when the ſulphur globe was uſed, it was ſhort, ſmall, and made a hiſſing noiſe. He alſo obſerved, that juſt the reverſe of both theſe caſes happened when he held the ſame wire in his hand, and the globes were worked alternately. The bruſh was large, long, diverging, and ſnapping or rattling, when the ſulphur globe was turned; but ſhort, ſmall, and hiſſing, when the glaſs globe was turned. When the bruſh was long, large, and much diverging, it ſeemed to the Doctor, that the body to which it joined was throwing the fire out, and when the contrary appeared, it ſeemed to be drinking in †.

3. He obſerved, that when he held his knuckle before the ſulphur globe, while it was turning, the ſtream of fire between his knuckle and the globe ſeemed to ſpread on its ſurface, as if it flowed from the finger, but before the glaſs globe it was otherwiſe.

4. He obſerved that the cool wind (or what was called ſo) which is felt as coming from an electrified point, was much more ſenſible when the glaſs globe, than when the ſulphur one was uſed. But theſe, though the beſt arguments which the ſenſes can furniſh, of the courſe of the electric fluid, the Doctor acknowledges were but haſty thoughts. Indeed, conſidering that the velocity of the electric fluid has been found, by experiment, to

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* Franklin's Letters, p. 104. † Ibid.

be nearly inſtantaneous, in a circuit of many miles, it cannot be ſuppoſed that the eye ſhould be able to diſtinguiſh which way it goes in the ſpace of one or two inches *.

I shall conclude this article with obſerving that the experiments, which the Doctor made with globes of glaſs and ſulphur, are much more eaſily exhibited by the conductor and inſulated rubber of either of them, all the effects being the reverſe of each other.

I must now, for the preſent, take leave of this ingenious writer and his friends, after having brought the hiſtory of their labours to the year 1754, and muſt return to ſee what was doing on the continent of Europe for two or three years preceding this date, while we left it to go over to America.

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* Franklin's Letters, p. 105.

PERIOD X.

THE HISTORY OF ELECTRICITY, FROM THE TIME THAT DR. FRANKLIN MADE HIS EXPERIMENTS IN AMERICA, TILL THE YEAR 1766.

WE are now entering upon the laſt period into which the hiſtory of electricity divides itſelf, in which the great variety of matter preſented to our view muſt oblige an hiſtorian to have recourſe to the ſtricteſt method; for, otherwiſe, the narration would be extremely perplexed and diſguſting. As this period contains the events of a larger ſpace of time than moſt of the others, yet without any convenient reſting place; as the buſineſs of electricity has been conſiderably multiplied in it, and a greater number of labourers have been employed in gathering in the harveſt of diſcoveries, the ſeeds of which were ſown by Dr. Watſon, Dr. Franklin, and others, in the preceding periods; I am obliged to ſubdivide this into more diſtinct parts, but I hope they will not be found to be more than were neceſſary, in order to prevent confuſion.

HOWEVER, this circumſtance, of the great quantity and variety of materials furniſhed in this period, in proportion as it tends to embarraſs an hiſtorian, and exerciſe his talent for proper diſtribution and arrangement, is a ſtriking demonſtration of a truth, which muſt give the greateſt pleaſure to all the lovers of electricity and Natural Philoſophy. If the progreſs continue the ſame in another period, of equal length, if the harveſt of diſcoveries continue to be more plentiful, and the labourers proportionably more numerous; what a glorious ſcene ſhall we ſee unfolded, what a fund of entertainment is there in ſtore for us, and what important benefits may be derived to mankind!

SECTION I.

IMPROVEMENTS IN THE ELECTRICAL APPARATUS, WITH EXPERIMENTS AND OBSERVATIONS RELATING TO IT.

AS our electrical apparatus has been much improved within this period, I ſhall firſt recite what has occurred to me upon this ſubject; particularly the methods which have, from time to time, been communicated of increaſing the power of electricity, by different circumſtances of excitation.

So early as the year 1751, upon occaſion of trying Mr. Winckler's experiments, notice is taken of Mr. Canton's method of rubbing tubes with ſilk prepared with linſeed oil. Theſe he had found, by the experience of ſome conſiderable time, to produce the greateſt effect upon tubes, but he had not found that they were proportionably uſeful in rubbing globes *.

UPON another occaſion, Mr. Canton obſerves, that by means of this rubber, a ſolid cylinder of glaſs, which had been ſet before the fire till it was quite dry, might be excited as eaſily as a glaſs tube, ſo as to act like one in every reſpect; that even the firſt ſtroke would make it ſtrongly electrical †.

BUT the greateſt improvement which Mr. Canton diſcovered for increaſing the power of electricity, was by rubbing on the cuſhion

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* Phil. Tranſ. Vol. xlvii. p. 239.

† Ibid. Vol. xlviii. pt. ii. p. 784.

cuſhion of the globe, or on the oiled ſilk rubber of the tube, a ſmall quantity of an amalgam of mercury and tin, with a very little chalk or whiting. By this means, a globe or tube may be excited to a very great degree with very little friction, eſpecially if the rubber be made more damp or dry as occaſion may require [*].

MR. WILCKE ſays, that a glaſs tube excited with a woollen cloth, on which ſome white wax or oil has been put, will throw out flames with a great noiſe in the dark [†]. Theſe flames, he ſays, he never knew to be thrown from a globe, except ſometimes when they were firſt uſed [‡].

OUR electrical apparatus has been much augmented within this period by the diſcovery of Father Windelinus Ammerſin of Switzerland, who, in a Latin treatiſe, publiſhed at Lucern, in the year 1754, has ſhewn us, that wood properly dried, till it becomes very brown, is a non-conductor of electricity. He recommends boiling the wood in linſeed oil, or covering it over with varniſh, after being dried, to prevent any return of moiſture into its pores; and adds, that wood, ſo treated, ſeems to afford ſtronger appearances of electricity than even glaſs. He himſelf made uſe of common wooden meaſures, ſuch as are uſually found in granaries, firſt boiled in oil, and afterwards mounted, ſo as to be turned by a wheel [§].

IT appears from the Philoſophical Tranſactions, ſays Mr. Wilſon, ſo early as the year 1747, that Dr. Watſon having occaſion to ſupport a long wire, in an experiment made near Shooter's Hill, with a view to determine the velocity of the electric fluid, uſed ſtakes of dry wood, which he told him, were baked, to prevent the electric fluid from eſcaping into the ground [||].

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[*] Phil. Tranſ. Vol. lii. pt. ii. p. 461.

[†] Wilcke, p. 124.

[‡] Ibid. p. 126.

[§] Phil. Tranſ. Vol. lii. pt. i. p. 342.

[||] Ibid. Vol. li. pt. ii. p. 896.

A MORE extraordinary method of procuring electricity than by baked wood, was one that Signior Beccaria made uſe of. He put a dry and warm cat's ſkin upon his glaſs globe, and rubbing it with his hand, excited a very powerful electrity *.

THESE wooden cylinders electrify poſitively or negatively as the rubber is ſilk or flannel, but much more powerfully when negative than when poſitive, owing to the roughneſs which there generally is upon their ſurfaces, and therefore make an agreeable variety in an electrical apparatus. But the oldeſt and moſt uſual method of procuring negative electricity was by globes of ſulphur. Theſe Mr. Le Roi made by putting a coating of ſulphur upon a globe of glaſs, and then ſmoothing it with an hot iron; but Mr. Nollet preferred melting the ſulphur in the inſide of the glaſs globe, and then breaking the glaſs from off it, becauſe this method made a much finer poliſh †.

ONE globe he made of a mixture of ſulphur and pounded glaſs, but he found that it had the ſame effect as if it had been all of ſulphur. He ſays that, when one part of this globe was excited, the whole ſurface became electrical ‡.

BUT ſince Mr. Canton's diſcovery of the negative power of rough glaſs, ſome philoſophers have made uſe of glaſs globes made rough by emery; and the uſual method of taking off their poliſh was by rubbing them as they turned upon their axis; but Mr. Speedler, a mathematical inſtrument maker at Copenhagen, obſerves, in his letters upon the ſubject of electricity, that glaſs globes, made rough by drawing the ſtone, or emery, from pole to pole, have a much greater virtue; this method of taking off the poliſh giving them a greater roughneſs with reſpect to the rubber §.

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* Lettere dell' Elettriciſmo, p. 58.

† Nollet's Letters, Vol. ii. p. 121.

‡ Ibid. p. 125, 127.

§ Wilcke, p 57.

BUT a better, and a readier method than all theſe of producing negative electricity, is by inſulating the rubber of a ſmooth globe, and connecting it with an inſulated prime conductor, while the common conductor hath a communication with the ground. The rubber, if well inſulated, is ſure to produce a negative electricity, equal in power to the poſitive of the ſame globe. Mr. Dalibard directs a great number of precautions, in order to electrify well at the rubber, and to prevent it from receiving any electric fire in its ſtate of inſulation *.

MR. BERGMAN of Upſal, ſays, that very often, when his glaſs globes could not be excited to a ſufficient degree of ſtrength, he lined them with a thin coating of ſulphur, and that then they gave a much ſtronger poſitive electricity than before †.

IN Italy, and other places, Mr. Nollet informs us, it is the cuſtom of electricians to put a coating of pitch, or other reſinous matter on the inſide of their globes, which, they pretend, makes them always work well ‡.

WE are obliged to the Abbé Nollet for ſome obſervations on the electrical powers of different kinds of glaſs, in the ſixth volume of his *Leçons de phyſique* printed in the year 1764.

IT is not every ſort of glaſs, ſays he, that is equally electrizable. There are ſome ſorts which are not ſo at all, or hardly at all; ſuch, for example, is that of which they make plates of glaſs at St. Gobin in Picardy. I have tried it, ſays he, an hundred times, in the form of plates, tubes, and globes, and in all kinds of weather, but have ſcarce been ever able to draw from it the leaſt ſenſible ſign of electricity.

THE glaſs of which panes for windows are made, and which is alſo uſed for drinking-glaſſes, when it is newly manufactured, is excited with great difficulty. I have often, ſays he, repeatedly rubbed tubes, and other pieces, even in the glaſs-houſe where they were made, but without ſucceſs; and it has not been till after ſome months, and ſometimes years, that I could bring them to act.

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* Dalibard's Franklin, p. 110.

† Phil. Tranſ. Vol. lii. pt. ii. p. 485.

‡ Lettres, Vol. ii. p. 122.

It is certain, and he ſays he has conſtantly obſerved, that glaſs becomes more fit for electrical experiments by force of rubbing and that ſometimes it has required ſome months to bring globes and tubes to act well.

He did not think that theſe facts could be accounted for either by the different degrees of tranſparency, or the different colours of glaſs. This, indeed, was evident from ſome globes acquiring electricity from uſe which had it not originally. The glaſs of which bottles are made at Severs ſerved him very well, whereas globes of white glaſs did not become tolerable till after having been uſed a certain time.

He could not tell poſitively why certain kinds of glaſs were electrizable or not by rubbing, but he ſuſpected, that it was principally owing to the degree of its hardneſs and vitrification. He was induced to think ſo, becauſe he found that the glaſs at the French manufactory at St. Gobin, and at Cherbourg (the hardeſt, the moſt compact, and the beſt vitrified of all the kinds of glaſs in France) was the moſt difficult to be electrized; whereas the cryſtal glaſs of England, that of Bohemia, &c. which are much ſofter, were the beſt of all for experiments in electricity. He ſays, moreover, that he had procured imperfect glaſſes, which had not been long enough in the furnace to be clear; and that, though they were of the ſame compoſition as plates of glaſs, which, he obſerved before, were not eaſily electrized, yet that theſe were excited very ſenſibly.

He ſays that a globe of ten or twelve inches diameter, and which makes about four revolutions in a ſecond of time, will receive a convenient rubbing; but that we muſt not expect that if the globe be one half, or one fourth part greater or leſs, the effects will be increaſed or diminiſhed in proportion *.

Upon the ſubject of inſulating bodies, he obſerves, that when the cakes of ſulphur, reſin, ſealing-wax, and bees-wax are made uſe of for this purpoſe, they ought to be well cooled before they are uſed: for, he ſays, he has conſtantly obſerved, that when they are newly made, they are not ſo proper to inſulate bodies, as they generally are at the end of ſome months †.

It will be proper, under this head, to acquaint young electricians, that globes have been ſeveral times known to burſt during the act of excitation, and that the fragments have been thrown with great violence in every direction, ſo as to be very dangerous for the by-ſtanders. This accident happened to Mr. Sabatelli in Italy, Mr. Nollet in France, Mr. Beraud at Lyons, Mr. Boze at Wittemburgh, Mr. Le Cat at Rouen, and Mr. Robein at Rennes.

The air in the inſide of Mr. Sabatelli's globe had no communication with the externul air, but that of the Abbé Nollet had. This laſt, which was of Engliſh flint, which had been uſed two years, and which was more than a line thick, burſt like a bomb in the hands of a ſervant who was rubbing it; and the fragments (the largeſt of which were not more than an inch in diameter) were diſperſed on all ſides, to a conſiderable diſtance. The Abbé ſays, that all the globes which were burſt in that manner exploded after five or ſix turns of the wheel; and he aſcribes this effect to the action of the electric matter, making the particles of the glaſs vibrate in a manner he could not conceive ‡.

When Mr. Beraud's globe burſt (and he was the firſt to whom this accident was ever known to happen) he was making ſome experiments

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* Leçons de Phyſique, Vol. vi. p. 273—276.

† Ibid. p. 299.

‡ Nollet's Letters, Vol. i. p 19.

experiments in the dark, on the 8th of February 1750; when a noiſe was firſt heard, as of ſomething rending to pieces; then followed the exploſion, and when the lights were brought in, it was obſerved, that thoſe places of the floor which were oppoſite to the equatorial diameter of the globe were ſtrewed with ſmaller pieces, and in greater numbers than thoſe which were oppoſite to other parts of it. This globe had been cracked, but it had been in conſtant uſe in that ſtate above a year, and the crack had extended itſelf from the pole to the equator. The proprietor aſcribed the accident to the vibration of the particles of the glaſs, and thought that the crack had ſome way impeded thoſe vibrations *.

WHEN Mr. Boze's globe broke, he ſays that the whole of it appeared in the act of breaking, like a flaming coal; a circumſtance which we ſhall ſee accounted for hereafter by Mr. Wilcke †.

MR. BOULANGER ſays, that glaſs globes have ſometimes burſt like bombs, and have wounded many perſons, and that their fragments have even penetrated ſeveral inches into a wall ‡. He alſo ſays, that if globes burſt in whirling by the gun-barrel's touching them, they burſt with the ſame violence, the ſplinters often entering into the wall §.

THE Abbé Nollet had a globe of ſulphur which alſo burſt, as he was rubbing it with his naked hands, after two or three turns of the wheel, having firſt cracked inwardly. It broke into very ſmall pieces, which flew to a great diſtance; and into a fine duſt, of which part flew againſt his naked breaſt; where it entered the ſkin ſo deep, that it could not be got off without the edge of a knife ||.

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* Hiſtoire, p. 87.

† Wilcke, p. 124.

‡ Boulanger, p. 23.

§ Ibid. p. 144.

|| Nollet's Letters, Vol. ii. p. 220.

SECTION

SECTION II.

OBSERVATIONS ON THE CONDUCTING POWER OF VARIOUS SUBSTANCES, AND PARTICULARLY MR. CANTON's EXPERIMENTS ON AIR; AND SIGNIOR BECCARIA's ON AIR, AND WATER.

ONE of the principal *deſiderata* in the ſcience of electricity, is to aſcertain wherein conſiſts the diſtinction between thoſe bodies which are conductors, and thoſe which are non-conductors of the electric fluid. All that has been done relating to this queſtion, till the preſent time, amounts to nothing more than obſervations, how near theſe two claſſes of bodies approach one another; and before the period of which I am now treating, theſe obſervations were few, general, and ſuperficial. But I ſhall now preſent my reader with ſeveral very curious and accurate experiments, which, though they do not give us intire ſatisfaction with reſpect to the great *deſideratum* above mentioned, yet throw ſome light upon the ſubject. They ſhow that ſubſtances which had been conſidered as perfect conductors, or non-conductors, are ſo only to a certain degree; and that, probably, all the known parts of nature have, in ſome meaſure, the properties of both.

THESE experiments were made by two perſons, whom, in the ſtyle of hiſtory, I may juſtly call two of the greateſt *heroes* of this part of my work, viz. Mr. CANTON, whoſe diſcoveries in electricity are far more numerous, and more conſiderable than thoſe of any other perſon, within this period, in England; and Signior BECCARIA, one of the moſt eminent of all the electricians abroad.

THAT air was capable of receiving electricity by communication, and of retaining it when received, had not been diſcovered by any perſon before Mr. Canton; but, by the help of one of his exquiſite contrivances, he was able to aſcertain that delicate circumſtance, and even meaſure the degree of it, if it was in the leaſt conſiderable.

HE got a pair of balls, turned in a lathe, out of the dry pith of elder. Theſe he put into a narrow box, with a ſliding cover, ſo diſpoſed that the threads (which were of the fineſt linen) were kept ſtraight in the box. Holding this box by the extremity of the cover, the balls would hang freely from a pin in the inſide. Theſe balls hung at a ſufficient diſtance from buildings, trees, &c. eaſily ſhow the electricity of the atmoſphere. They alſo determine whether the electricity of the clouds and the air be poſitive, by the decreaſe; or negative, by the increaſe of their repulſion, at the approach of excited amber or ſealing-wax.

BY the help of this inſtrument, he obſerved, that it was poſſible to electrify the air of a room near the apparatus; and even the air of the whole room in which it was, to a conſiderable degree, and he was able to do it both poſitively and negatively.

IN a paper read at the Royal Society, December the 6th, 1753, he obſerves, that the common air of a room might be electrified to a conſiderable degree, ſo as not to part with its electricity for ſome time. Having rendered the air of his room very dry, by means of a fire, he electrified a tin tube (with a pair of balls ſuſpended at one of its extremities) to a great degree; when it appeared, that the neighbouring air was likewiſe electrified. For, having touched the tube with his finger, or any other conductor, the balls, notwithſtanding, continued to repel one another, though not at ſo great a diſtance as before *. But he obſerves that their repulſion would decreaſe as they were moved towards the floor, wainſcot, or any of the furniture; and that they would touch each other when brought within a ſmall diſtance of any conductor. Some degree of this electric power, he has known to continue in the air above an hour after the rubbing of the tube, when the weather had been very dry.

To electrify the air, or the moiſture contained in it, negatively, Mr. Canton ſupported, by ſilk ſtretched between two chairs (placed back to back, at the diſtance of about three feet), a tin tube with a fine ſewing needle at one end of it; and rubbed ſulphur, ſealing-wax, or a rough glaſs tube as near as he could to the other end, for three or four minutes; after which he found the air to be negatively electrical, and that it would continue ſo a conſiderable time after the apparatus was removed into another room †.

In a paper dated November the 11th, 1754, he ſays, that dry air, at a great diſtance from the earth, if in an electric ſtate, will continue ſo till it meets with ſome conductor, is probable from the following experiment. An excited glaſs tube, with its natural poliſh, being placed upright in the middle of a room (by putting one end of it into an hole, made for that purpoſe, in a block of wood) would, generally, loſe its electricity in leſs than five minutes, by attracting to it a ſufficient quantity of moiſture, to conduct the electric fluid from all parts of its ſurface to the floor; but if, immediately after it was excited, it was placed, in the ſame manner, before a good fire, at the diſtance of about two feet, where no moiſture would adhere to its ſurface, it

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* Phil. Tranſ. Vol. xlix. pt. i. p. 300.

† Ibid. Vol. xlviii. pt. ii. p. 784.

would continue electrical a whole day, and how much longer he knew not [*].

SINCE the publication of the firſt edition of this work, Mr. Canton has hit upon another, much readier, and more powerful method of communicating electricity to the air than that deſcribed above. This he gives me leave to publiſh, and it appears to me to be of ſuch a nature, as that it may very poſſibly lead to farther diſcoveries concerning the electricity of the atmoſphere, and the phenomena depending upon it. "Take," ſays he, a "charged phial in one hand, and a lighted candle, inſulated, in "the other; and, going into any room, bring the wire of the "phial very near to the flame of the candle, and hold it there "about half a minute: then carry the phial and candle out of "the room, and return with the pith balls, ſuſpended, and held "at arm's length. The balls will begin to ſeparate on enter-"ing the room, and will ſtand an inch and half, or two inches "a part, when brought near the middle of it."

SIGNIOR BECCARIA, who had no knowledge of what Mr. Canton had done, made the ſame diſcovery of the communication of electricity to the air, and diverſified the experiment in a much more pleaſing and ſatisfactory manner. He proves, that the air, which is contiguous to an electrified body, acquires, by degrees, the ſame electricity; that this electricity of the air counteracts that of the body, and leſſens its effects, and that as the air acquires, ſo it alſo parts with this electricity very ſlowly.

HE began his experiments by hanging linen threads upon an electrified chain, and obſerving, that they diverged the moſt after a few turns of his globe. After that, they came nearer together, notwithſtanding he kept turning the globe and the excitation was as powerful as ever [†].

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[*] Phil. Tranſ. Vol. xlviii. pt. ii. p. 784.

[†] Lettere dell' Elettriciſmo, p. 87.

WHEN he had kept the chain electrified a conſiderable time, and then diſcontinued the friction, the threads collapſed by degrees, till they hung parallel. After this, they began to diverge again, without any freſh electrification; and, if the air was ſtill, this ſecond divergence would continue an hour, or more.

THIS divergence was leſſened by the electrification of the chain. For if the globe was turned again, the threads would firſt become parallel, and then begin to diverge again as before. Thus the ſecond divergence of the threads took place, when the chain was deprived of its electricity, and when that which the air had acquired began to ſhow itſelf.

WHILE the threads were beginning to diverge with the electricity of the air, if he touched the chain, and thereby took off what remained of its electricity, the threads would ſeparate farther. Thus the more the electricity of the chain was leſſened, the more did the electricity of the air appear.

WHILE the threads were in their ſecond divergence, he hung two other threads, ſhorter than the former, by another ſilk thread to the chain; and when all the electricity of the chain was taken quite away, they would ſeparate, like the former threads.

IF he preſented other threads to the former, in their ſecond divergence, they would all avoid one another *.

IN this complete and elegant manner did Signior Beccaria demonſtrate, that air actually receives electricity by communication, and loſes it by degrees; and that the electricity of the air counteracts that of the body which conveys electricity to it.

SIGNIOR BECCARIA alſo made a variety of other experiments, which demonſtrate other mutual affections of the air and the electric fluid; particularly ſome that prove their mutual re-

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* Lettere dell' Elettriciſmo, p. 90.

pulſion; and that the electric fluid, in paſſing through any portion of air, makes a temporary vacuum.

He brought the ends of two wires within a ſmall diſtance of one another, in a glaſs tube, one end of which was cloſed, and the other immerged in water; and obſerved, that the water ſunk in the tube, every time that a ſpark paſſed from the one to the other, the electric fluid having repelled the air *.

He made the electric exploſion a great number of times, in the ſame air, incloſed in a glaſs tube, in order to aſcertain whether the elaſticity of the air was affected by it; but he could not find any alteration. After the operation, he broke the tube under water, but neither did any air make its eſcape, nor any water force its way into the tube. The experiment was made with all the precaution, with reſpect to heat and cold, that the nature of the caſe required †.

Signior Beccaria's experiments on *water*, ſhowing its imperfection as a conductor, are more ſurpriſing than thoſe he made upon air, ſhowing its imperfection in the contrary reſpect. They prove that water conducts electricity according to its quantity, and that a ſmall quantity of water makes a very great reſiſtance to the paſſage of the electric fluid.

He made tubes, full of water, part of the electric circuit, and obſerved, that when they were very ſmall, they would not tranſmit a ſhock, but that the ſhock increaſed as wider tubes were uſed ‡.

But what aſtoniſhes us moſt in Signior Beccaria's experiments with water, is his making the electric ſpark viſible in it, notwithſtanding its being a real conductor of electricity. Nothing, however, can prove more clearly how imperfect a conductor it is.

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* Elettriciſmo artificiale e naturale, p. 110. † Ibid. p. 81. ‡ Ibid. p. 113.

HE inſerted wires, ſo as nearly to meet, in ſmall tubes filled with water; and, diſcharging ſhocks through them, the electric ſpark was viſible between their points, as if no water had been in the place. The tubes were generally broken to pieces, and the fragments driven to a conſiderable diſtance. This was evidently occaſioned by the repulſion of the water, and its incompreſſibility, it not being able to give way far enough within itſelf, and the force with which it was repelled being very great*.

THE force with which ſmall quantities of water are thus repelled by the electric fluid, he ſays, is prodigious. By means of a charge of four hundred ſquare inches, he broke a glaſs tube two lines thick, when the pieces were driven to the diſtance of twenty feet. Nay he ſometimes broke tubes eight or ten lines thick, and the fragments were driven to greater diſtances in proportion †.

HE found the effect of the electric ſpark upon water greater than the effect of a ſpark of common fire on gunpowder; and ſays he does not doubt, but that, if a method could be found of managing them equally well, a cannon charged with water would be more dreadful than one charged with gunpowder. He actually charged a glaſs tube with water, and put a ſmall ball into it, when it was diſcharged with great force, ſo as to bury itſelf in ſome clay which he placed to receive it ‡.

THIS reſiſtance which ſmall quantities of water make to the electric matter, he imagined, was greater than the reſiſtance made to it by air §. And yet he thought it was poſſible, that, in this caſe, the electric matter might not act upon the water immediately, but upon the fixed air that was in it. For when the tubes were not broken, he obſerved that a great number of air bubbles were let looſe, through the whole maſs of the water, roſe to the top, and mixed with the common atmoſphere [*].

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* Elettriciſmo artificiale e naturale, p. 114.

† Lettre dell' elettriciſmo, p. 74.

‡ Ibid. p. 75, 76. Mr. LULLIN ſays, he produced much greater effects than theſe, by making the electric ſpark viſible in *oil* inſtead of water. Oil being a much worſe conductor, the ſpark in it would be larger. Diſſertatio Phyſica, p. 26.

§ Elettriciſmo artificiale, &c. p. 115.

HE alſo imagined that the electric fluid acted upon the fixed air in all bodies, though no experiment could make it ſenſible [†].

ON the contrary, he ſuppoſed that the action of the electric matter tended to fix elaſtic air, by exciting a ſulphureous matter, which Dr. Hales ſhows to have that property [‡]. But the experiment above-mentioned, of the electric ſpark taken in a cloſed tube, doth not favour this ſuppoſition.

WHEN a ſmall drop of water was put between the points of two wires, and a large ſhock paſſed through them, the water was equally diſperſed on the inſide of a glaſs ſphere, in which they were all incloſed. In the ſame manner, he conjectures, that the action of the electric matter promotes the evaporation of water [§].

DISCHARGING a ſhock through a quantity of water, poured on a flat ſurface, where ſome parts of the circuit were purpoſely left almoſt dry; thoſe parts became quite dry ſooner than they would have been, if no ſhock had paſſed through them [‖].

UPON this principle he accounts for the ſuppoſed burſting of the blood veſſels in ſmall birds killed by the electric ſhock [¶]. And when a muſcle contracts by the ſhock, he ſuppoſes it is owing to the dilatation of the fluids their fibres contain, as the electric matter paſſes through them.

So imperfect a conductor of electricity is mere water, that, he thought, a green leaf conducted a ſhock better than an equal thickneſs of water [⸸]. If this be true, and vegetable fluids conduct electricity better than water, it will confirm a conjecture which Dr. Franklin told me he had drawn from ſome experiments that he had not properly purſued, viz. that animal fluids conducted electricity better than water. He tried milk many years ago; and Mr. Kinnerſley, and others in America, have ſince tried blood and urine, and alſo the ſinews of animals newly killed; and they were all found to be exceedingly good conductors, remarkably better than water.

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[*] Elettriciſmo artificiale, &c. p. 116.
[†] Ibid. p. 83.
[‡] Ibid.
[§] Ibid. p. 117.
[‖] Ibid. p. 121.
[¶] Ibid. p. 128.
[⸸] Ibid. p. 135.

Signior Beccaria alſo found, that even *metal* was not a perfect conductor of electricity, but made ſome reſiſtance to the paſſage of the electric fluid. This he aſcertained, by meaſuring the time that it was retarded, in its paſſing through long and ſmall wires, notwithſtanding the experiments which had been made before, that ſeemed to prove the contrary.

He ſuſpended a wire of five hundred Paris feet, in a large building, and, by means of a pendulum which vibrated half ſeconds, obſerved, that light bodies placed under a ball of gilt paper, at one end, did not move, till, at leaſt, one vibration of this pendulum, after he had applied the wire of a charged phial to the other.

Trying the ſame with a hempen cord, he could count ſix, or more vibrations before they would ſtir; but when he had wetted the cord, they were moved after two or three vibrations*. He does not, however, abſolutely ſay, that the electric fluid muſt have taken up all this time in its progreſs, as it might require a certain quantity of the fluid, before it could raiſe the light bodies. But he did imagine, that it moved with more velocity, in proportion as the bodies into which it paſſed had more or leſs of the fluid before†. And he was confirmed in this opinion by ſeveral phenomena of the atmoſphere, which will be related in their proper place, particularly by ſeeing, very evidently, the progreſs of a quantity of electric matter in the air, as it advanced to ſtrike his kite.

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* Elettriciſmo artificiale, &c. p. 51.

† Ibid.

To theſe experiments of Signior Beccaria on the conducting powers of air and water, I ſhall ſubjoin another curious ſet of the ſame author, ſhowing the manner in which the ſmoke of roſin and of colophonia is affected by the approach of an electrified body, as they have a very near affinity to this ſubject.

Repeating Dr. Franklin's experiments to make electric atmoſpheres viſible with the fume of colophonia, which he preferred, for this purpoſe, to roſin; he obſerved ſeveral curious circumſtances, which had eſcaped the notice of that ingenious philoſopher.

He heated the colophonia on a coal, which he held in a ſpoon under an electrified cube of metal; and obſerved, that when part of the ſmoke aſcended to the cube, another part covered the handle of the ſpoon, and ſpread to his hand *.

The ſmoke lay higher on the flat parts of the cube than on the edges, and corners.

If a ſpark was taken from the conductor, the ſmoke was thrown into an agitation, but preſently reſumed its former ſituation.

The cube with its atmoſphere gave larger, and longer ſparks, than a cube not ſurrounded with one.

A larger ſpark might be taken from it by the ſpoon, than by any other body.

Having inſulated the ſpoon, he obſerved, that hardly any of the ſmoke went to the cube; and that what happened to go near it was not affected by it, any more than it would have been by any other body. He put his finger to the ſpoon,

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* Elettriciſmo artificiale, p. 72.

and the former phenomena returned. Taking it off again, the ſmoke that had ſettled on the cube ſoon diſperſed [*].

Under this head of the electricity of various ſubſtances, it will not be improper to mention an experiment made by Mr. Henry Eeles of Liſmore in Ireland, which, he thought, proved that ſteam, and exhalations of all kinds, are electrical. The paper containing this account was read at the Royal Society, April the 23d, 1755.

He electrified a piece of down, ſuſpended on the middle of a long ſilk ſtring, and made ſteam and ſmoke of ſeveral kinds paſs under it, and through it; and obſerved, that its electricity was not in the leaſt diminiſhed, as he thought it would have been, if the vapour had been non-electric, and conſequently had taken away with it part of the electric matter with which the down was loaded. He obſerved that the effect was the ſame, whether the down was electrified with glaſs or wax, which he thought was not eaſy to be accounted for [†].

To this experiment Dr. Darwin of Litchfield, in a letter addreſſed to the Royal Society, and read May the 5th, 1757, anſwers; that many electrified bodies, and particularly all light, dry, animal, and vegetable ſubſtances, will not eaſily part with their electricity, though they be touched, for a conſiderable time, with conductors. He touched a feather, electrified like that of Mr. Eeles, nine times with his finger, and ſtill found it electrified. A cork ball was touched ſeven times in ten ſeconds before it was exhauſted [‡].

Mr. Kinnersley of Philadelphia, in a letter dated March 1761, informs his friend and correſpondent Dr. Franklin, then in England, that he could not electrify any thing by means of *ſteam* from electrified boiling water; from whence he concluded, that, contrary to what had been before ſuppoſed by himſelf and his friend, ſteam was ſo far from riſing electrified, that it left its ſhare of common electricity behind *.

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[*] Elettriciſmo artificiale, p. 73, 74.

[†] Phil. Tranſ. Vol. xlix. pt. i. p. 153.

[‡] Ibid. Vol. l. pt. i. p. 252.

To try the effects of electricity upon air, Mr. Kinnerſley contrived an excellent inſtrument, which he calls *an electrical air thermometer*. It conſiſted of a glaſs tube, about eleven inches long, and one inch in diameter, made air tight, cloſed with braſs caps at each end, and a ſmall tube, open at both ends, let down through the upper plate, into ſome water at the bottom of the wider tube. Within this veſſel he placed two wires, one deſcending from the braſs cap at the upper end, and the other aſcending from the braſs cap at the lower end; through which he could diſcharge a jar, or tranſmit a ſpark, &c. and at the ſame time ſee the expanſion of the air in the veſſel, by the riſe of the water, in the ſmall tube. With this inſtrument he made the following experiments, related in a letter to Dr. Franklin, dated March the 12th, 1761.

He ſet the thermometer on an electric ſtand, with the chain fixed to the prime conductor, and kept it well electrified a conſiderable time; but this produced no conſiderable effect: from whence he inferred, that the electric fire, when in a ſtate of reſt, had no more heat than the air, and other matter wherein it reſides.

When the two wires within the veſſel were in contact, a large charge of electricity, from above thirty ſquare feet of coated glaſs, produced no rarefaction in the air; which ſhowed, that the wires were not heated by the fire paſſing through them.

When the wires were about two inches aſunder, the charge of a three pint bottle, darting from one to the other, rarefied the air very evidently; which ſhewed, that the electric fire produced heat in itſelf, as Mr. Kinnerſley ſays, as well as in the air, by its rapid motion.

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* Phil. Tranſ. Vol. liii. pt. i. p. 84.

The charge of a jar which contained about five gallons and a half, darting from wire to wire, would cauſe a prodigious expanſion in the air; and the charge of his battery of thirty ſquare feet of coated glaſs would raiſe the water in the ſmall tube quite to the top. Upon the coaleſcing of the air, the column of water, by its gravity, inſtantly ſubſided, till it was in equilibrio with the rarefied air. It then gradually deſcended, as the air cooled, and ſettled where it ſtood before. By carefully obſerving at what height the deſcending water firſt ſtopped, the degree of rarefaction, he ſays, might be diſcovered, which, in great exploſions, was very conſiderable.

It is obvious to remark, that the firſt ſudden riſe of the water of Mr. Kinnerſley's thermometer, upon an exploſion being made in the veſſel which contained it, is not to be aſcribed to the rarefaction of the air by heat, but to the quantity of air actually diſplaced by the electrical flaſh. It is only when that firſt ſudden riſe is ſubſided, as Mr. Kinnerſley himſelf obſerves, that the degree of its rarefaction by the heat can be eſtimated, viz. by the height at which the water then ſtands above the common level.

Dr. Franlkin had ſaid, that *ice* failed to conduct a ſhock of electricity; and Mr. Bergman, in a letter to Mr. Wilſon, read at the Royal Society November the 20th, 1760, ſhows (what Signior Beccaria had done before) that a ſmall quantity of water failed as much as the ice had done with Dr. Franklin, who ſeems to have made uſe of an icicle which, Mr. Bergman thought, was not large enough for the purpoſe. From hence he ſuſpected, that large quantities of ice would tranſmit a ſhock of electricity as perfectly as a large quantity of water *.

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* Phil. Tranſ. Vol. li. pt. ii. p. 908.

HOWEVER, he ſeems, afterwards, to have changed his ſentiments with reſpect to ice: for, in a ſubſequent paper, read at the Royal Society March the 18th, 1762, when he had remarked that ſnow would not conduct the electric ſhock, he ſays, he believes, if he could procure plates of ice of a proper thickneſs, he could charge them in the ſame manner as glaſs *.

JOHANNES FRANCISCUS CIGNA was ſo fully perſuaded of the non-conducting power of ice, that he made uſe of it in an experiment, deſigned to aſcertain whether electric ſubſtances did, according to Dr. Franklin's hypotheſis, contain more of the electric matter than other bodies. He incloſed a quantity of ice in a glaſs veſſel, and when he thought he had converted it from an electric to a non-electric by melting; he tried whether it was electrified; but, though he found no appearance of its having acquired any more of the fluid than it ought to have in its new ſtate, he does not ſeem to have given up his opinion †.

IN the laſt part of this work the reader will find ſome experiments, which, it is imagined, will aſcertain the claſs of bodies in which ice ought to be ranked, by proving its conducting power to be, at leaſt, nearly equal to that of water.

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* Phil. Tranſ. Vol. lii. pt. ii. p. 485.

† Memoirs of the Academy at Turin, for the year 1765, p. 47.

SECTION III.

Mr. CANTON's EXPERIMENTS AND DISCOVERIES RELATING TO THE SURFACES OF ELECTRIC BODIES, AND OTHERS MADE IN PURSUANCE OF THEM, OR RELATING TO THE SAME SUBJECT; ALL TENDING TO ASCERTAIN THE DISTINCTION BETWEEN THE TWO ELECTRICITIES.

TILL this laſt period of the hiſtory, the ſame electricity had always been produced by the ſame electric. The friction of glaſs had always produced a poſitive, and the friction of ſealing-wax, &c. had always produced a negative electricity. Theſe were thought to be eſſential, and unchangeable properties of thoſe ſubſtances; and hence the one was by many called the vitreous, and the other the reſinous electricity; and to electrify negatively, that is, produce a reſinous electricity, by means of glaſs; or to electrify poſitively, that is, produce a vitreous electricity, by means of ſealing-wax, &c. would have been thought as great a paradox, as to electrify at all by the friction of braſs or iron. For though it was not known why the electric matter ſhould flow from the rubber to the excited glaſs, or to the rubber from excited ſealing-wax, the fact had been invariable; and nothing is even mentioned to have happened, in the courſe of any experiments, that could lead a perſon to ſuſpect the poſſibility of the contrary.

WHAT then muſt have been the ſurprize of electricians, to find that theſe different powers of glaſs and ſulphur were ſo far from being invariable, that they were even interchangeable; and that the ſame glaſs tube could be made to aſſume the powers of both! And what muſt have been their ſatisfaction to find the circumſtance on which the convertibility of thoſe oppoſite powers depended, completely aſcertained. This ſurprize and pleaſure was given them by Mr. Canton, who ſhowed that it depended only on the rubber, and the ſurface of the glaſs, whether it electrified poſitively or negatively.

IN what manner, by what train of thought, or by what accident he was led to this diſcovery, this excellent philoſopher has not been pleaſed to inform us; but it is certainly a diſcovery which, in an eminent manner, diſtinguiſhes this period of my hiſtory. It throws great light upon the doctrine of poſitive and negative electricity, and led the way to other diſcoveries which throw ſtill more light upon it.

THIS ſubject of the two electricities ſeems to have engaged the attention of electricians in a more particular manner, in the whole courſe of this period, and ever ſince the diſcovery of Dr. Franklin, that the electricity of the two ſurfaces of charged glaſs are always contrary to one another. Accordingly, the reader will find ſeveral ſections in this period of the hiſtory relating to it; but he will find that though much has been done, much yet remains to be done; and that we are ſtill far from thoroughly underſtanding the nature of the two electricities, with their dependence upon and relation to one another.

PREVIOUS to the communication of the diſcovery itſelf, Mr. Canton obſerves, that ſealing-wax might have poſitive electricity ſuperinduced upon it. He excited a ſtick of ſealing-wax about two feet and a half in length, and an inch in diameter; and, holding it by the middle, he drew an excited glaſs tube ſeveral times over one part of it, without touching the other. The conſequence was, that that half which had been expoſed to the action of the excited glaſs was poſitive, and the other half negative: for the former half deſtroyed the repelling power of balls electrified by glaſs, while the other half increaſed it *.

The experiments, which prove that the appearances of poſitive and negative electricity depend upon the ſurface of the electrics, and that of the rubber, were made in the latter end of December 1753.

Having rubbed a glaſs tube with a piece of thin ſheet lead, and flour of emery mixed with water, till its tranſparency was intirely deſtroyed, he excited it (when it was made perfectly clean and dry) with new flannel, and found it act in all reſpects like excited ſulphur or ſealing-wax. The electric fire ſeemed to iſſue from the knuckle, or end of the finger, and to ſpread itſelf on the ſurface of the tube, in a very beautiful manner.

If this rough or unpoliſhed tube was excited by a piece of dry oiled ſilk, eſpecially when rubbed over with a little chalk or whiting, it would act like a glaſs tube with its natural poliſh. In this caſe the electric fire appeared only at the knuckle, or the end of the finger, where it ſeemed to be very much condenſed, before it entered.

But if the rough tube was greaſed all over with tallow from a candle, and as much as poſſible of it wiped off with a napkin, then the oiled ſilk would receive a kind of poliſh by rubbing it; and, after a few ſtrokes, would make the tube act in the ſame manner as when excited at firſt by flannel.

The oiled ſilk, when covered with chalk or whiting, would make the greaſed rough tube act again like a poliſhed one; but

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* Phil. Tranſ. Vol. xlviii. pt. i. p. 356.

if the friction was continued till the rubber became ſmooth, the electric power would be changed to that of ſulphur, ſealing-wax, &c.

Thus, ſays he, may the poſitive and negative powers of electricity be produced at pleaſure, by altering the ſurfaces of the tube and rubber, according as the one or the other is moſt affected by the friction between them. For if the poliſh be taken off one half of a tube, the different powers may be excited with the ſame rubber at a ſingle ſtroke; and, he adds, the rubber is found to move much eaſier over the rough, than over the poliſhed part of it.

That poliſhed glaſs electrified poſitively, and rough glaſs rubbed with flannel negatively, ſeemed plain from the appearance of the light between the knuckle, or end of the finger, and the reſpective tubes. But this, Mr. Canton thought, was farther confirmed by obſerving, that a poliſhed glaſs tube, when excited by ſmooth oiled ſilk, if the hand was kept three inches, at leaſt, from the top of the rubber, would, at every ſtroke, appear to throw out a great number of diverging pencils of electric fire; but that none were ever ſeen to accompany the rubbing of ſulphur, ſealing-wax, &c. nor was he ever able to make any ſenſible alteration in the air of a room merely by the friction of thoſe bodies; whereas the glaſs tube, when excited ſo as to emit pencils, would, in a few minutes, electrify the air, to ſuch a degree, that, after the tube was carried away, a pair of balls, about the ſize of the ſmalleſt peas, turned out of cork, or the pith of elder, and hung to a wire by linen threads of ſix inches long, would repel each other to the diſtance of an inch and an half, when held at arm's length in the middle of the room *.

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* Phil. Tranſ. Vol. xlviii. pt. ii. p. 782.

AFTER theſe experiments of Mr. Canton, Mr. Wilſon made ſeveral, which throw a little more light upon this curious ſubject; but it is difficult to draw any general concluſion from them, and his own is not ſufficiently determinate. It is, that two electrics being rubbed together, the body whoſe ſubſtance is hardeſt, and electric power ſtrongeſt, will always be *plus*, and the ſofteſt and weakeſt *minus* *. Rubbing the tourmalin and amber together, he produced a *plus* electricity on both ſides of the ſtone, and a *minus* on the amber; but rubbing the tourmalin and diamond together, both ſides of the tourmalin were electrified *minus*, and the diamond *plus*.

THESE experiments, which, he thought, proved this propoſition, encouraged him to try what would be the effect of rubbing or forcing air againſt different electrics, and the effects were very conſiderable. In theſe experiments he only made uſe of a common pair of bellows, and his firſt experiment was upon the tourmalin. This ſubſtance he brought near the end of the pipe, and found, that after it had received about twenty blaſts, it was electrified *plus* on both ſides. Air, therefore, ſeemed to be leſs electric than the tourmalin.

INTO the place of the tourmalin, he brought a pane of glaſs, and blew againſt it the ſame number of times as in the former experiment; and when he had examined both ſides, he found that they were electrified *plus* alſo, but leſs than the tourmalin.

AMBER, treated in the ſame manner, was electrified leſs than the glaſs.

HE next had recourſe to a ſmith's bellows. The difference which theſe occaſioned was only a much ſtronger electricity in the tourmalin. Amber was ſtill weaker than the glaſs; and the glaſs weaker than the tourmalin.

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* Phil. Tranſ. Vol. li. pt. i. p. 331.

HAVING in view the medium (which, I have obſerved, he laid great ſtreſs upon, as conſtituting the difference between electrics and non-electrics) he conſidered that heat would rarify it on the ſurfaces of the particles of air; by which means, air, having its reſiſtance leſſened, would more readily part with the electric fluid, and, of conſequence, electrify more powerfully.

THE pipe of the bellows being made red-hot, he blew againſt the tourmalin, twelve times only, which was eight times leſs than in the former experiment with cold air. In this experiment the tourmalin was electrified *plus* on both ſides, but to a conſiderable degree more than was done in the former experiments. The hot air had the ſame effect upon glaſs, but electrified it leſs than the tourmalin; and amber, though, like the other bodies, it received an increaſe of power by the ſame treatment, was electrified the leaſt of all.

FROM the air electrifying more powerfully when it was hot than when it was cold, and the tourmalin being electrified more than glaſs, and glaſs more than amber, as appeared by the laſt experiments, we ſeem, ſays Mr. Wilſon, to have obtained a proof, that the whole atmoſphere is conſtantly promoting a flow of the electric fluid, by the alternate changes of heat and cold; and farther, that air is not only leſs electric than the tourmalin, but leſs than glaſs, or even amber *.

IN another paper, read at the Royal Society November the 13th, 1760, Mr. Wilſon recites ſome curious experiments, which, he ſays, ſhew that a *plus* electricity may be produced by means of a *minus* electricity.

HAVING electrified the inſide of a large Leyden bottle *plus*, by means of a conducting wire from an excited glaſs globe; he ſet it on a ſtand of prepared wood, and took away the conducting wire, after which the mouth of the bottle was cloſed with a ſtopple of glaſs. Then the pointed end of an ivory conductor was brought oppoſite to the middle of the bottle, and about two inches from it. Upon doing this, the balls were electrified *minus*; and the more ſo as the ivory was moved nearer the bottle, in an horizontal direction.

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* Phil. Tranſ. Vol. li. pt. i. p. 332, &c.

But, on removing the ivory to a greater diſtance, the *minus* electricity decreaſed; and, at a certain diſtance, there was not any ſign of it remaining; but when the diſtance was increaſed to about eighteen inches from the bottle, a *plus* electricity appeared, which continued even after the ivory was removed entirely away*.

With a cylinder of baked wood he electrified the balls hanging to the ivory *minus*, at the diſtance of four feet or more, by holding the cylinder over the middle of the ivory, and continuing it there; and, on moving it nearer, they were more ſtrongly electrified *minus*; but the ſame cylinder, on removing it back again to the diſtance of two or three feet, or more, electrified the balls *plus*.

When another conductor of metal, without edges or points, was uſed, inſtead of the ivory, and without any thing hanging from it, the ſame cylinder held over the metal (as was done in the laſt experiment over the ivory, at the diſtance of two feet) produced a *plus* electricity; and this was rendered weaker as the cylinder was moved nearer; but by leſſening the diſtance to about one foot, the *minus* electricity took place. In theſe caſes Mr. Wilſon thought, that the *plus* appearance aroſe from the earth, air, or other neighbouring bodies.

When the preceding experiments were firſt made, he was a little embaraſſed, by the uncertain appearances of a *plus* electricity at one time, and a *minus* at another, in the ſame experiment; but, by repeated trials and obſervations, he found, that a *plus* or *minus* electricity may be produced at pleaſure, by carefully attending to the three following circumſtances; viz. the form of the bodies, their ſudden or gradual removal, and the degrees of electrifying.

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* Phil. Tranſ. Vol. li. pt. ii. p. 899, &c.

MR. WILSON, after this, proceeds to mention ſome other circumſtances of a very nice nature, where, the ſlighteſt and almoſt imperceptible differences in the poſition or in the courſe of the friction of two bodies produce, in either of them, the *plus* electricity at one time, and the *minus* at another. Such, ſays he, are the effects of this ſubtle and active fluid, when the experiments are carefully made; and therefore they require the moſt ſcrupulous attention to trace out the cauſes which occaſion them.

SEALING-WAX and ſilver were the bodies uſed in the two firſt experiments, but many other ſubſtances ſeemed to perform as well. The ſealing-wax was clean, and undiſturbed by any friction whatever, but that of the air ſurrounding it, and had been ſo for ſome hours. The ſilver was fixed to a piece of prepared wood, which was alſo preſerved from friction for the ſame length of time. Then, taking one of thoſe ſubſtances in each hand, the ſilver being at the end of the wood the fartheſt from the hand, he laid the ſmootheſt part of the ſilver upon the ſealing-wax, and moved it along the ſurface gently, once only, and with a very ſlight preſſure, after which the ſilver was electrified *plus*, and the wax *minus*.

ON repeating the experiment with equal care, and in the ſame manner, except that the ſmooth ſide of the ſilver was a little inclined, ſo that the edge of it preſſed againſt the wax; the ſilver, after moving it as before, was electrified *minus*, and the wax *plus*, contrary to what was obſerved in the laſt experiment.

THESE oppoſite effects, occaſioned by the different applications of the *flatted part* or *edge* of the ſilver, ſeemed to ariſe from an alteration made in the ſurface of the wax, by deſtroying the poliſh in one caſe, and not in the other; and in this reſpect reſembled the poliſhed and rough glaſs mentioned before.

UPON making uſe of prepared wood inſtead of wax, and employing different degrees of preſſure in the friction, with the ſame edge of the ſilver, he produced the like appearances; the leaſt preſſure cauſing a *plus*, and the greateſt preſſure a *minus* appearance in the ſilver.

A FLAT piece of ſteel well poliſhed, and the edges rounded off, afforded the ſame appearances, by only applying the flat ſurface to the wood, but it required more preſſing to produce the *minus* effect in this caſe than it did in the former, where the edge was concerned.

WHETHER the reaſon offered above for explaining theſe laſt curious appearances be true or not, Mr. Wilſon did not venture to affirm, for want of farther experiments; but thus much he thought might be ſafely advanced, that we have learned to produce at pleaſure a *plus* or *minus* electricity from the ſame bodies, by attending to the manner of their application and friction *.

MR. BERGMAN, in a letter to Mr. Wilſon, read at the Royal Society February the 23d, 1764, gives an account of ſome curious experiments of his, which, in conjunction with thoſe of Mr. Canton above mentioned, concerning ſurfaces, may throw conſiderable light upon the doctrine of poſitive and negative electricity.

THE experiments were made with two ribbons of ſilk, one of which was extended in a frame, while Mr. Bergman held the other in his hand. He obſerved, that if the two ribbons were the ſame with reſpect to texture, colour, ſuperficies, and in every thing elſe, as far as could be judged; and if he drew the whole length of the ribbon which he held in his hand over one part of that which was extended in the frame, that in his hand contracted the poſitive electricity, and that in the frame a negative. If he drew one part of that which he held in his hand over the whole length of the other, the effects were reverſed.

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* Phil. Tranſ. Vol. li. pt. ii. p. 899, &c.

If the ribbon in his hand was of a different colour from that in the frame (provided it was not black) the event was the ſame.

If the ribbon in his hand was black, it was always negative, which ever way it was rubbed, except that in the frame was black too; for then, if the whole length of it was rubbed, it was electrified poſitively.

In endeavouring to account for theſe effects, he obſerves, that the ribbon which was moſt rubbed was made *ſmoother*, and *warmer* than the other; and was of opinion, that though ſmoothneſs did diſpoſe bodies to be excited poſitively, yet that other circumſtances were alſo to be taken into conſideration; having found that when he held in his hand a ribbon, which, by much friction, was made very ſmooth, and drew it over one part of another ribbon, which was rough, and had never been uſed before, that the rough ribbon was, nevertheleſs, poſitive. From this experiment he concluded, that this effect was, in ſome meaſure, owing to the colour; and, in purſuing this thought farther, he was led to the following experiments.

If the ribbon in his hand was well warmed, though it was drawn over one part of that in the frame, it became electrified negatively, and that in the frame poſitively. He made theſe experiments with the ſame ſucceſs upon ribbons of ſilk of various colours, blue, green, red, white, &c.

If the ribbon in the frame was black, it never contracted a poſitive electricity, though that in his hand had been much heated, except this were black too. From theſe experiments, he thought he might ſafely conclude, that heat did diſpoſe ſome ſubſtances, at leaſt, to a negative ſtate; and he thought that the want of attention to this circumſtance might have occaſioned miſtakes in the event of ſome experiments, eſpecially thoſe concerning iſland cryſtal.

From the whole he concludes, that there is a certain fixed order with reſpect to negative and poſitive electricity, in which all bodies may be placed, while other circumſtances remain the ſame. Let A, B, C, D, E, be certain ſubſtances, each of which, when rubbed with one which is antecedent to it, is negative, but with a ſubſequent poſitive. In this caſe, the leſs diſtance there is between the bodies that are rubbed, the weaker, *cæt. par.* will be the electricity; wherefore it will be ſtronger between A and E, than it will be between A and B. Heat, he ſays, diſpoſes bodies to a negative electricity, but if the diſtance above-mentioned be conſiderable, it may not be able to *overcome*, though it may *weaken* that electricity, as is evident from the ribbon of black ſilk. When a glaſs globe grows warm in whirling, we are ſenſible that its electric power is diminiſhed. Is it not owing, ſays he, to this circumſtance, that by heat it is more diſpoſed to negative electricity, by which means the diſtance above-mentioned between the glaſs and the rubber is leſſened [*]?

Upon the ſubject of this ſection, I muſt introduce to the acquaintance of my reader two eminent electricians whoſe diſcoveries will give him the greateſt ſatisfaction; I mean Mr. Wilcke, and Mr. Æpinus, the former of Roſtock in Lower Saxony, and the latter of Peterſburgh: a circumſtance which gives me an occaſion of congratulating all the lovers of the ſciences, and particularly of electricity, on the extenſive ſpread of their ſtudies.

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[*] Phil. Tranſ. Vol. liv. p. 86.

What joy would it have given Mr. Haukſbee, or Mr. Grey, to have foreſeen that two ſuch admirable treatiſes on the ſubject of electricity, as thoſe of the perſons above mentioned, would come from countries ſo remote from the place of its riſe!

MR. WILCKE relates many curious experiments concerning the generation of what he calls *ſpontaneous electricity*, produced by the liquefaction of electric ſubſtances, which, compared with thoſe of Mr. Canton, throw great light upon the doctrine of poſitive and negative electricity.

HE melted ſulphur in an earthen veſſel, which he placed upon conductors; then, letting them cool, he took out the ſulphur, and found it ſtrongly electrical; but it was not ſo when it had ſtood to cool upon electric ſubſtances.

HE melted ſulphur in glaſs veſſels, whereby they both acquired a ſtrong electricity in the circumſtances above-mentioned, whether they were placed upon electrics or not; but a ſtronger in the former caſe than in the latter; and they acquired a ſtronger virtue ſtill, if the glaſs veſſel into which they were poured was coated with metal. In theſe caſes, the glaſs was always poſitive, and the ſulphur negative. It was particularly remarkable, that the ſulphur acquired no electricity till it began to cool and contract, and was the ſtrongeſt when in the ſtate of greateſt contraction; whereas the electricity of the glaſs was, at the ſame time, the weakeſt; and was the ſtrongeſt of all when the ſulphur was ſhaken out, before it began to contract, and acquired any negative electricity.

PURSUING theſe experiments, he found that melted ſealing-wax, poured into glaſs, acquired a negative electricity, but poured into ſulphur it acquired a poſitive electricity, and left the ſulphur negative. Sulphur poured into baked wood became negative. Sealing-wax alſo poured into wood was negative, and the wood conſequently poſitive; but ſulphur poured into ſulphur, or into rough glaſs, acquired no electricity at all *.

Experiments ſimilar to theſe were alſo made by Mr. Æpinus. He poured melted ſulphur into metal cups, and obſerved that when the ſulphur was cold, the cup and the ſulphur together ſhowed no ſigns of electricity, but ſhowed very ſtrong ſigns of it the moment they were ſeparated. The electricity always diſappeared when the ſulphur was replaced in the cup, and revived upon being taken out again. The cup had acquired a negative, and the ſulphur a poſitive electricity; but if the electricity of either of them had been taken off while they were ſeparate, they would both, when united, ſhow ſigns of that electricity which had not been taken off. This electricity, he obſerves, was only on the ſurface of the ſulphur †.

Mr. Wilcke has, likewiſe, recited ſeveral curious experiments, which he made on the friction of various ſubſtances, which likewiſe throw conſiderable light on the ſame ſubject.

Sulphur and glaſs rubbed together produced a ſtrong electricity, poſitive in the glaſs, and negative in the ſulphur.

Sulphur and ſealing-wax being rubbed together, the wax became poſitive, and the ſulphur negative.

Wood rubbed with cloth was always negative.

Wood rubbed againſt ſmooth glaſs became negative, but againſt rough glaſs poſitive.

Sulphur rubbed againſt metals was always poſitive, and this was the only caſe in which it was ſo; but being rubbed againſt lead it became negative, and the metal poſitive; lead appearing, thereby, to be not ſo good a conductor as the other metals.

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* Wilcke, p. 44, &c.

† Æpini Tentamen, p. 66. 70.

AFTER theſe experiments, Mr. Wilcke gives the following catalogue of the principal ſubſtances with which electrical experiments are made, in the order in which they are diſpoſed to acquire poſitive or negative electricity; any of the ſubſtances becoming poſitively electrical when rubbed with any that follow it in the liſt, and negative when rubbed with any that precede it.

Smooth glaſs.
Woollen cloth.
Quills.
Wood.
Paper.
Sealing-wax.
White wax.
Rough glaſs.
Lead.
Sulphur.
Other metals *.

IN all experiments made to determine the order of theſe ſubſtances, Mr. Wilcke ſays, that great care is neceſſary, to diſtinguiſh original electricity from that which is communicated, or the conſequence of friction †.

MR. WILCKE ſays that ſmooth glaſs is in all caſes poſitive, and thence infers that it attracts the electric fluid the moſt of all known ſubſtances; but Mr. Canton tells me he has found, that the ſmootheſt glaſs will acquire a negative electricity by being drawn over the back of a cat.

OF the ſame nature with theſe experiments of Mr. Wilcke are the following of Æpinus. He preſſed cloſe together two pieces of looking-glaſs, each containing ſome ſquare inches; and obſerved, that when they were ſeparated, and not ſuffered to communicate with any conductor, they each acquired a ſtrong electricity, the one poſitive, and the other negative. When they

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* Wilcke, p. 54, &c.

† Ibid. p. 69.

were put together again, the electricity of both diſappeared, but not if either of them had been deprived of their electricity when they were aſunder; for in that caſe, the two when united, had the electricity of the other. The ſame experiment, he ſays, may be made with glaſs and ſulphur, or with any other electrics, or with any electric and a piece of metal [*].

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[*] *Æpini Tentamen*, p. 65.

SECTION

SECTION IV.

MR. DELAVAL'S EXPERIMENTS RELATING TO THE TWO ELECTRICITIES, AND HIS CONTROVERSY WITH MR. CANTON UPON THAT SUBJECT.

MR. CANTON, in the courſe of experiments related in the preceding ſection, clearly proved, that the production of either of the two electricities depends intirely upon the ſurface of the excited electric with reſpect to the rubber, and ſhowed, that the very ſame glaſs tube would produce either of them at pleaſure; yet, notwithſtanding this demonſtration, Mr. Delaval, ſeveral years afterwards, propoſed another theory of the two electricities, which ſeems to be more ingenious than ſolid; as it goes upon the old ſuppoſition of the different powers depending intirely upon the different ſubſtances themſelves. The account of this theory was read at the Royal Society, March the 22d, 1759. It neceſſarily occaſioned ſome controverſy with Mr. Canton, in the courſe of which ſome new experiments were made, and ſome new facts diſcovered; on account of which I ſhall, with the utmoſt impartiality, report all that was advanced on both ſides.

MR. DELAVAL obſerved, that there are two of the pure chymical principles of bodies, viz. *earth* and *ſulphur*, which are each poſſeſſed of a different kind of electricity; one of which we might call a *plus* electricity, the other a *minus*; and thought that it might be expected, that, in a body compounded of both, the oppoſite powers of thoſe ingredients would counterbalance, and deſtroy the effect of each other; and therefore, that bodies in which the negative and poſitive powers were equal, would be neutral, or non-electrics. Such a ſubſtance he took metal to be, conſiſting of calx and ſulphur; metals not being calcinable without a degree of heat ſufficient to diſſipate all their ſulphur; as is evident from their not being reducible again to their metallic form, without the admixture of ſome unctuous matter. The ſame diſſipation of ſulphur, he ſays, muſt take place in animal and vegetable ſubſtances, before they become white aſhes. Tranſparent ſtones he conſidered as little more than pure earth, free from the leaſt mixture of oil; judging of others by the chymical reſolution of cryſtal.

To confirm this theory, Mr. Delaval made experiments with dry powders of calcined metals, viz. ceruſs, lead aſhes, minium, calx of antimony, &c. incloſing them in long glaſs tubes, and endeavouring to tranſmit the electric virtue through them, and always finding it impoſſible. Animal and vegetable ſubſtances, when reduced to aſhes, were alike impermeable to electricity, as alſo the ruſt of metals.

He was firſt led to theſe experiments, and to this hypotheſis, by finding that dry mould would not conduct electricity. This he alſo tried with dry Portland ſtone, ſome of which he had cut into plates nearly as thin as window glaſs. Theſe he heated to a proper degree, and coated them on both ſides with metal, in order to make the Leyden experiment. When the ſtone was hot enough to ſinge paper, it conducted as perfectly as when cold; but on cooling a little, it began not to conduct, and afforded ſmall ſhocks; which gradually increaſed in ſtrength for about ten minutes, at which time it was about its moſt perfect ſtate, and remained ſo near a quarter of an hour. After that time, the ſhocks gradually decreaſed, as the ſtone grew cooler; till, at laſt, they ceaſed, and the ſtone returned to its conducting ſtate again, but this ſtate appeared before the ſtone was quite cold.

EXPERIMENTS of this kind ſucceeded with all bodies abounding with calx, or earth, as ſtones, earth, dry clay, wood when rotten, or burned in the fire till the ſurface becomes black. Among other ſubſtances he tried a common tobacco-pipe, part of which, near the middle, he heated to a proper degree, and then applied one end of it to an electrical bar, while the other was held in the hand; and he obſerved, that the electric fluid paſſed no farther along the pipe than to the heated part *.

FROM theſe experiments Mr. Delaval inferred, that ſtones and other earthy ſubſtances were convertible, by ſeveral methods, and particularly by different degrees of heat, from non-electrics to electrics. But finding, afterwards, that it was the opinion of ſome perſons (Mr. Canton was the perſon chiefly hinted at) that this change did not immediately, but only conſequentially depend on heat, by evaporating the moiſture, which would return again when the ſubſtance cooled; he obſerves, in a paper read at the Royal Society, December the 17th, 1761, that the tobacco-pipe loſt its electricity before it was cold, and therefore before it could have imbibed moiſture ſufficient to deſtroy its electricity; and beſides, that the ſubſtance employed in the experiment was not of that kind of bodies which is apt ſuddenly to draw moiſture from the air.

To account for Mr. Delaval's experiments, Mr. Canton ſuppoſes, in a paper read to the Royal Society, February 4th, 1762, that ſtone, tobacco-pipe, wood, &c. will conduct when cold by the moiſture they contain in that ſtate; that when their moiſture

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* Phil. Tranſ. Vol. li. pt. i. p. 83.

is evaporated by heat they become non-conductors; and that when they are made very hot, the hot air at, or near their ſurfaces will conduct, and the bodies will appear to be conductors again. Hot air, he ſays, may eaſily be proved to be a conductor of electricity, by bringing a red-hot iron poker, but for a moment, within three or four inches of a ſmall electrified body; when it would be perceived, that its electric power would be almoſt, if not intirely deſtroyed; and by bringing excited amber within an inch of the flame of a candle, when it would loſe its electricity before it had acquired any ſenſible degree of heat.

To confirm this, he mentions his having obſerved, that the tourmalin, Braſil topaz, and Braſil emerald, would give much ſtronger ſigns of electricity when cooling, after they had been held about a minute within two inches of an almoſt ſurrounding fire, where the air is a conductor, than they ever will after heating them in boiling water. He adds, that if both ſides of thoſe ſtones be equally heated, in a leſs degree than will make the ſurrounding air a conductor, the electricity of each ſide, whether *plus* or *minus*, would continue ſo all the time the ſtone was both heating and cooling, but would increaſe while it was heating, and decreaſe while it was cooling; whereas, if the heat was ſufficient to make the ſurrounding air conduct the electric fluid from the poſitive ſide of the ſtone to the negative ſide of it, while it was heating, the electricity of each ſide would increaſe while the ſtone was cooling, and be contrary to what it was while the ſtone was heating.

As to the tobacco-pipe, Mr. Canton ſays, that it not only attracts the moiſture of the air, but abſorbs it. Hence a tobacco-pipe, after it begins to cool, will become a conductor again ſooner than wood. And that it imbibes moiſture faſter than wood, he ſays, is evident, becauſe when wetted, it will not continue wet ſo long as wood, imbibing the moiſture preſently.

THAT tobacco-pipe does not become a conductor by a particular degree of heat, without evaporating its moiſture, is evident, he ſays, from the following experiments.

IF three or four inches of one end of a tobacco-pipe, of more than a foot in length, be made red-hot, without ſenſibly heating the other end, this pipe will prove a ready conductor, through the hot air ſurrounding one part of it, and the moiſture contained in the other; although ſome part of it muſt have the degree of heat of a non-conductor. But if the whole pipe be made red-hot, and ſuffered to cool till it has only ſuperficial moiſture enough to make it a good conductor, and then three or four inches of one end be again made hot, it will become a non-conductor.

IF a nail be placed at, or near each end of a longiſh ſolid piece of any of the abſorbent bodies above-mentioned, ſo as the point of each nail may be about half the thickneſs of the body within its ſurface; this body, by heat, may be made a non-conductor externally or ſuperficially, while it remains a good conductor internally. For the electric fluid will readily paſs from one nail to the other, through the middle of the body, when it will not paſs on its ſurface, and even when the internal parts of the body are in an equal degree of heat with the external, as they muſt ſoon be after it begins to cool. But if the ſame body be expoſed, for a ſhort time, to a greater degree of heat than before, or if it be kept longer in the ſame heat, it will become a non-conductor intirely *.

Mr. DELAVAL, in confirmation of particular bodies requiring particular degrees of heat to render them electric or non-electric, independent of moiſture, mentions a ſubſtance which, he ſays, is affected by heat in an oppoſite manner to the former inſtances;

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* Phil. Tranſ. Vol. lii. pt. ii. p. 459.

ſince the degrees of heat, neceſſary to render the other ſubſtances electric, makes this non-electric.

THE ſubſtance was *iſland cryſtal* (which is well known for its ſingular property of a double refraction) on a piece of which he made the following obſervations. 1. After this piece of cryſtal had been rubbed, when the heat of the air was moderate, it ſhowed ſigns of electricity, though not very ſtrong ones. 2. If the heat was increaſed, ſo as to be a little greater than that of the hand, it deſtroyed its electric power intirely. 3. By cooling the ſtone again, the electric power was reſtored.

HE immerſed this piece of cryſtal into a veſſel filled with quickſilver, and ſurrounded with ice, where it remained near two hours, when the weather was very cold; and obſerved, that, upon taking it out with a pair of tongs (that it might not be altered by the heat of his hands) and rubbing it again, it was more ſtrongly electric than he had at any other time experienced; but that, on placing it a few minutes upon the hearth, at ſome diſtance from the fire, its electric property was again deſtroyed, for that rubbing would not occaſion any ſigns of it.

THUS, ſays he, we ſee two different kinds of fixed bodies, the one of which acquires an electric property with the ſame heat with which another loſes it; while a third ſet of ſubſtances, as glaſs, &c. retain their electricity through both the degrees of heat neceſſary to the other two.

SOME pieces of iſland cryſtal, which he had procured from different places, had not the property of loſing their electricity by a moderate heat. He had, in particular, a piece of that cryſtal, one part of which, when greatly heated, became non-electric, while the other part, with the ſame heat, or even with a much greater one, remained perfectly electric.

HE found ſeveral other earthy ſubſtances, whoſe electricity was deſtroyed by different degrees of heat.

FROM conſidering that the degree of heat, at which the iſland cryſtal firſt mentioned was in its moſt perfect electric ſtate, was leſs than the uſual heat of the air, and that a ſmall increaſe of that heat rendered it non-electric; he did not think it improbable, that many ſubſtances, which are not known to be electric, might prove ſo, if expoſed to a greater degree of cold than they have been hitherto examined in[*].

To theſe obſervations Mr. Canton replies, that having formerly obſerved that the friction between mercury and glaſs in vacuo would not only produce the light of electricity, as in the luminous barometer, or within an evacuated glaſs ball, but would alſo electrify the glaſs on the outſide, he immerged a piece of dry glaſs in a baſon of mercury; and found, that by taking it out, the mercury was electrified *minus*, and the glaſs *plus*, to a conſiderable degree. He alſo found, that amber, ſealing-wax, and iſland cryſtal, when taken out of mercury, were all electrified poſitively. How then, ſays he, does it appear, that the electricity which was obſerved in rubbing the laſt mentioned ſubſtance, after it was taken out of mercury ſurrounded by ice, was owing to cold, and not to the friction between it and the mercury in taking it out. Iſland cryſtal when warm is a non-conductor, and all non-conductors may be excited with proper rubbers[†].

MR. BERGMAN of Upſal, in a letter to Mr. Wilſon, read at the Royal Society, April the 14th, 1761, ſays, that he had tried the experiments of Mr. Delaval with iſland cryſtal, but that the event had always been contrary to what Mr. Delaval had reported. Trying different pieces of cryſtal, he found one which inſtead of having its virtue increaſed by cooling, was ſenſibly increaſed by heating. Afterwards trying all the reſt which he had by him, whether Swediſh cryſtal, or iſland, he found the effect to be the ſame. From this he inferred, that the cryſtals which he had were of a quite different kind from that of Mr. Delaval *.

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[*] Phil. Tranſ. Vol. lii. pt. i. p. 354, &c.

[†] Ibid. pt. ii. p. 461.

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* Phil. Tranſ. Vol. liii. pt. i. p. 98.

SECTION V.

MR. CANTON'S EXPERIMENTS AND DISCOVERIES RELATING TO BODIES IMMERGED IN ELECTRIC ATMOSPHERES, WITH THE DISCOVERIES OF OTHERS, MADE BY PURSUING THEM.

IN this ſection I ſhall preſent my reader with the fineſt ſeries of experiments that the whole hiſtory of electricity can exhibit, and in which we ſhall ſee diſplayed the genius and addreſs of four of the moſt eminent electricians in this whole period; viz. Mr. Canton and Dr. Franklin, Engliſhmen; and Meſſrs. Wilcke and Æpinus, foreigners. Mr. Canton had the honour to take the lead, and he made all the eſſential experiments. Doctor Franklin profeſſedly purſued them; and though *all his ſtrength he put not forth* on this occaſion, he diverſified the experiments, and made ſome improvement in the method of accounting for them. But Meſſrs. Wilcke and Æpinus in conjunction carried the experiments vaſtly farther, and completed the diſcovery; which is, certainly, one of the greateſt that has been made ſince the time of Dr. Franklin. I ſay the time of Dr. Franklin, though he himſelf be one of the perſons concerned; for by the *time of Dr. Franklin* will always be underſtood the time in which he made his capital diſcoveries in America. This will always be a diſtinguiſhed epocha in the hiſtory of electricity, from which all his own future diſcoveries will be dated.

The original experiments in this ſection, when Mr. Canton firſt publiſhed them, in his uſual conciſe, though perſpicuous manner, without any preamble, to inform us how he was led to them, exhibit ſuch a variety of attractions and repulſions of electrified bodies in different circumſtances, as looked like the power of magic; and were they conducted with a little art, I do not know any electrical experiments (made without light, or noiſe) more proper for a deception of this kind. But when they are attentively conſidered, they demonſtrate a remarkable property of all electrified bodies, which has often been referred to in the courſe of this hiſtory, but which had not been attended to before; nor indeed do I apprehend that it was fully underſtood, till it was explained in all its extent by Mr. Wilcke and Æpinus. It is, that the electric fluid, when there is a redundancy of it in any body, repels the electric fluid, in any other body, when they are brought within the ſphere of each other's influence, and drives it into the remote parts of the body; or quite out of it, if there be any outlet for that purpoſe. In other words, bodies immerged in electric atmoſpheres always become poſſeſſed of the electricity, contrary to that of the body, in whoſe atmoſphere they are immerged. This principle purſued led them to the method of charging a plate of air, like a plate of glaſs, and to make the moſt perfect imitation of the phenomena of thunder and lightning.

The paper, containing an account of Mr. Canton's experiments, was read at the Royal Society, December the 6th, 1753.

Mr. Canton ſuſpended cork balls, one pair by linen threads, and another pair by ſilk; then holding the excited tube at a conſiderable diſtance from the balls with the linen thread, they ſeparated; and, upon drawing it away, they immediately came together: but he was obliged to bring the excited tube much nearer to the balls hanging by ſilk threads, before they would ſeparate; though when the tube was withdrawn, they continued ſeparate for ſome time.

As the balls in the former of theſe experiments were not inſulated, Mr. Canton obſerves, that they could not properly be ſaid to be electrified; but that when they hung within the atmoſphere of the excited tube, they might attract and condenſe the electric fluid round about them, and be ſeparated by the repulſion of its particles. He conjectures alſo, that the balls, at this time, contain leſs than their common ſhare of the electric fluid, on account of the repelling power of that which ſurrounds them, though ſome may be continually entering and paſſing through the threads. And if that be the caſe, he ſays, the reaſon is plain why the balls hung by ſilk in the ſecond experiment muſt be in a much more denſe part of the atmoſphere of the tube before they will repel each other. He adds, that at the approach of an excited ſtick of wax to the balls, in the firſt experiment, the electric fire is ſuppoſed to come through the threads into the balls, and to be condenſed there, in its paſſage towards the wax; ſince, according to Dr. Franklin, excited glaſs emits the electric fluid, and excited wax receives it.

When two balls, ſuſpended by linen threads upon an inſulated tin tube, were electrified poſitively, and had ſeparated; he obſerved, that the approach of the excited tube would make them come nearer together; if brought to a certain diſtance, they would touch; and if brought nearer, they would ſeparate again.

In the return of the tube, they would approach each other, till they touched, and then repel as at firſt. If the tin tube was electrified by wax, or the wire of a charged phial; the balls would be affected in the ſame manner at the approach of excited wax, or the wire of the phial. If the cork balls were electrified by glaſs, their repluſion would be increaſed at the approach of an excited ſtick of wax. And the effect would be the ſame, if the excited glaſs was brought towards them, when they had been electrified by wax.

THE bringing the excited glaſs to the end, or edge of the tin tube, in the former of theſe experiments, is by Mr. Canton ſuppoſed to electrify it poſitively, or to add to the electric fire it before contained; and therefore ſome will be running off through the balls, and they will repel each other. But at the approach of excited glaſs, which likewiſe emits the electric fluid, the diſcharge of it from the balls will be diminiſhed, or part will be driven back, by a force acting in a contrary direction, and they will come nearer together. If the tube be held at ſuch a diſtance from the balls, that the exceſs of the denſity of the fluid round about them above the common quantity in air, be equal to the exceſs of the denſity of that within them above the common quantity contained in cork, their repulſion will be quite deſtroyed. But if the tube be brought nearer, the fluid without being more denſe than that within the balls, it will be attracted by them, and they will recede from each other again.

MR. CANTON farther obſerves, that when the apparatus has loſt part of its natural ſtore of this fluid, by the approach of excited wax to one end of it, or is electrified negatively, the electric fire is attracted and imbibed by the balls, to ſupply the deficiency; and that more plentifully at the approach of excited glaſs, or a body poſitively electrified, than before; whence the diſtance between the balls will be increaſed, as the fluid ſurrounding them is augmented. And, in general, whether by the approach or receſs of any body, if the difference between the denſity of the internal and external fluid be increaſed, or diminiſhed; the repulſion of the balls will be increaſed, or diminiſhed accordingly.

HE obſerved, that when the inſulated tin tube was not electrified, if the excited glaſs was brought towards the middle of it, the balls hanging at the end would repel each other, and the more ſo as the excited tube was brought nearer. When it had been held a few ſeconds, at the diſtance of about ſix inches, and withdrawn, the balls would approach each other till they touched; and, ſeparating again, as the tube was removed farther, would continue to repel when the tube was taken quite away. This laſt repulſion would be increaſed by the approach of excited glaſs, and diminiſhed by that of excited wax; juſt as if the apparatus had been electrified by wax, after the manner deſcribed in the laſt experiment.

HE inſulated two tin tubes, which may be diſtinguiſhed by calling them A and B, ſo as to be in a line with each other, and half an inch aſunder, and at the remote end of each ſuſpended a pair of cork balls. Then, upon bringing the excited glaſs tube towards the middle of A, and holding it a ſhort time at the diſtance of a few inches, he obſerved each pair of balls to ſeparate. Upon withdrawing the tube, the balls of A would come together, and then repel each other again, but thoſe of B would hardly be affected. By the approach of excited glaſs the repulſion of the balls of A would be increaſed, and thoſe of B diminiſhed *.

IN the former of theſe experiments, Mr. Canton ſuppoſes the common ſtock of electric matter in the tin tube to be attenuated about the middle, and to be condenſed at the ends, by the repelling power of the atmoſphere of the excited glaſs tube, when held near it. And perhaps, he ſays, the tin tube may loſe ſome of its natural quantity of the electric fluid before it receives any from the glaſs, as that fluid will more readily run off from the ends or edges of it than enter at the middle; and accordingly, when the glaſs tube is withdrawn, and the fluid is again equally

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* Phil. Tranſ. Vol. xlviii. pt. i. p. 350.

diffuſed through the apparatus, it is found to be electrified negatively; ſince excited glaſs brought under the balls will increaſe their repulſion.

In the latter of the experiments, Mr. Canton ſuppoſes that part of the fluid driven out of one tin tube enters the other, which is found to be electrified poſitively, by the decreaſing of the repulſion of its balls at the approach of excited glaſs.

It will readily be ſeen that, at the time theſe experiments were made, Mr. Canton retained the common idea of electric atmoſpheres; whereas it will appear by the experiments of Meſſrs. Wilcke and Æpinus (which in fact contain nothing more than thoſe of Mr. Canton) that they tend to refute the common opinion, and are much eaſier explained upon the ſuppoſition, that the portion of fluid belonging to any electrified body is conſtantly held in contact, or very nearly in contact, with the body; but acts upon the electricity of other bodies at a certain diſtance.

Dr. Franklin purſued, or rather diverſified the experiments of Mr. Canton, but retaining, likewiſe, the common opinion of electric atmoſpheres, he thought that the phenomena were more eaſily explained upon the ſuppoſition, that theſe atmoſpheres, being brought near each other, did not eaſily mix, and unite into one atmoſphere, but remained ſeparate, and repelled each other; and moreover, that an electric atmoſphere, would not only repel another electric atmoſphere, but alſo the electric fluid contained in the ſubſtance of a body approaching it, and, without joining or mixing with it, force it into the other parts of the body that contained it.

Though it muſt be difficult to aſſign a reaſon why the particles of one atmoſphere ſhould repel the particles of another atmoſphere, or of the fluid contained in another body with more force than they repel one another, or the particles of the fluid contained in the body to which they belong, ſince the matter is the ſame in both; yet this idea of the mutual repulſion of electric atmoſpheres, could it once be ſuppoſed, will certainly and clearly account for all the facts; and the theory pleaſes on account of its ſimplicity. But the ſame appearances will be accounted for, in a manner as ſimple and intelligible, upon the ſuppoſition, that the portion of electric fluid belonging to each body, being ſtrongly attracted by the body, is held in cloſe contact with it; but that it acts by repulſion upon the electric fluid belonging to other bodies, at a diſtance from them; and that the electric fluid doth not actually paſs out of one body into another, till it have firſt repelled the fluid out of the other body, and then be more ſtrongly attracted by the other body, than by its own; which has already got more than its natural ſhare.

THE paper containing an account of theſe experiments of Dr. Franklin was read at the Royal Society, December the 18th, 1755. His apparatus was different from that of Mr. Canton, but ſtill he exhibited the ſame effects proceeding from the ſame cauſe. He fixed a taſſel of fifteen or twenty threads, each three inches long, at one end of his prime conductor, which was five feet long and four inches in diameter, ſupported by ſilk lines. The threads were a little damp, but not wet.

IN theſe circumſtances, an excited tube brought near the end of the prime conductor, oppoſite to the threads, ſo as to give it ſome ſparks, made the threads diverge, each thread having thereby acquired its ſeparate electric atmoſphere.

IN this ſtate, the approach of the excited tube, without giving any ſparks, made the threads diverge more; but, being withdrawn, they cloſed as much; the atmoſphere of the conductor being driven by that of the tube into the threads, and returning again upon withdrawing the tube, which had then left no part of its atmoſphere behind it.

The excited tube brought under the diverging threads made them cloſe a little, having driven part of their atmoſpheres into the conductor. Upon being withdrawn, they diverged as much; that portion of their atmoſpheres which they had loſt returning again from the conductor, and the tube having left no part of its own.

The excited tube, held at the diſtance of five or ſix inches from the end of the conductor oppoſite to the threads, made them ſeparate, and, upon being withdrawn, they came together again: but if, in their ſtate of ſeparation, a ſpark was taken from the conductor near them, they would cloſe; and, upon removing the tube, would ſeparate. The tube, in both caſes, left no part of its atmoſphere behind it. It only drove the natural quantity of electricity contained in the conductor towards the threads; and part of that being taken away by the ſpark, the tube would leave the conductor and threads negative, in which caſe, they would repel one another, as if they had been electrified poſitively.

In this ſituation, if the excited tube was brought near the conductor, they would cloſe again; the atmoſphere of the tube forcing that of the conductor into the threads, to ſupply the place of what they had loſt: but, upon withdrawing the tube, they would open again; the tube, as before, taking its whole atmoſphere away with it. When the excited tube was brought under the threads, diverging with negative electricity, they diverged more; the atmoſphere of the tube driving away more of the atmoſpheres of the threads, and giving them none in its place.

Lastly the Doctor brought the excited tube near the prime conductor, when it was not electrified; and when the threads were, thereby, made to diverge, he brought his finger near them, and obſerved, that they receded from it. This appearance had been taken notice of by Mr. Haukſbee, and others. Dr. Franklin accounts for it by ſuppoſing, that when his finger was plunged ed into the atmoſphere of the glaſs tube, part of its natural electricity was driven back, through his hand and body, ſo as to leave the finger negatively electrified, as well as the threads; in which caſe they muſt neceſſarily repel one another. To confirm this hypotheſis, he held a ſlender lock of cotton, two or three inches long, near the prime conductor, electrified by excited glaſs, which made the cotton ſtretch itſelf towards the conductor; and obſerved, that, in this ſtate, it would recede from the finger of his other hand, at the ſame time that it was attracted by a wire of a bottle charged poſitively*.

THESE experiments of Dr. Franklin, made in purſuance of thoſe of Mr. Canton, were confirmed, as I obſerved before, and carried much farther by Meſſrs. Wilcke and Æpinus.

MR. WILCKE obſerves, that a ſmall body immerged in any electric atmoſphere, if it be touched by no other body, and be withdrawn before it be repelled, ſcarce ever ſhows any ſign of electricity; if any, it is of the ſame kind with that of the body into whoſe atmoſphere it was plunged †. If any body, communicating with the ground, be brought to this light body, while it remains immerged in the atmoſphere of the electrified body, it is firſt attracted, and then repelled by it. If a point be preſented to this light body, and afterwards withdrawn, it will be found to have acquired an electricity oppoſite to that of the electrified body. From this he concludes, that parts of non-electric bodies, plunged in electric atmoſpheres, acquire an electricity oppoſite to that of the atmoſphere in which they are plunged ‡.

HE placed two large inſulated conductors with their ends oppoſite to one another, and a cork ball ſuſpended on ſilk between them; and obſerved, that, upon the application of the excited glaſs tube to one end of either of them, the cork ball would play between them very faſt; and if the tube were held a while at the ſame diſtance, would be at reſt. Upon withdrawing the tube, the motion of the cork ball began again, and, at length, ceaſed gradually as before. If the conductors were removed from one another, while they were within the atmoſphere of the tube, they would, upon being brought together again, give a ſpark. This experiment confirmed the demonſtration, that the part of a body which is immerged in the atmoſphere of an electrified body acquires the contrary electricity *.

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* Phil. Tranſ. Vol. xlix. pt. i. p. 300.

† Wilcke, p. 73.

‡ Ibid. p. 77.

BUT the moſt complete demonſtration of this general maxim is an experiment of Mr. Æpinus. He placed a ſmall weight upon one end of a large metallic conductor, and, by means of a ſilk ſtring, removed it from the conductor, while the end on which it reſted was immerged in the atmoſphere of an electrified body; and found that it had actually acquired a different electricity from that of the atmoſphere. If the end of the conductor, oppoſite to that on which the moveable weight was placed, was made to communicate with the earth, ſtill that part of it which was near the excited electric was affected with the oppoſite electricity. Placing the moveable weight on the oppoſite end of the conductor, when it was inſulated, he found that it had ſometimes acquired an electricity contrary to that of the excited electric, ſometimes the ſame electricity, though weak, and ſometimes no electricity at all †.

THE ſame ingenious philoſopher conſidered that the ſame principle muſt extend to glaſs, and all other electrics; ſince they, as well as conductors, contain a certain quantity of the electric fluid, in their natural ſtate. To verify this, he took a glaſs tube, and electrified one end of it poſitively. The conſequence was, that four or five inches of that end were poſitive;

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* Wilcke, p. 78.

† Æpini Tentamen, p. 129.

but beyond that there were two inches negative; and beyond that the tube was again poſitive, though weakly ſo. This experiment he repeated very often with the ſame ſucceſs; as alſo when, inſtead of glaſs, he uſed a ſolid ſtick of ſulphur. To account for this fact, he ſuppoſed, that the electricity communicated to the end of the tube repelled the natural quantity of the fluid in the glaſs to ſome diſtance. This natural quantity retiring from its former ſituation, he ſuppoſes to become condenſed, and conſequently to repel another quantity of the fluid natural to the glaſs from its place; and that thus the whole rod would be alternately poſitive and negative. The author aſſerts, that it was from theory only that he was led to this curious experiment, the fact exactly correſponding to what he had before deduced, as the neceſſary conſequence of Dr. Franklin's principles of negative and poſitive electricity*.

The hint of theſe experiments Mr. Æpinus received from thoſe above mentioned of Mr. Wilcke; and theſe gentlemen, reſiding at the ſame time at Berlin, purſued theſe curious experiments jointly, till they were led by them to diſcover a method of charging a plate of air in the ſame manner as plates of glaſs had uſually been charged, and to throw ſtill more light upon the theory of the famous Leyden experiment.

In the above mentioned experiments, theſe gentlemen obſerved, that the negative ſtate of one of the bodies depended on the oppoſite ſtate of the other, which was known to be exactly the caſe of the two ſides of a charged pane of glaſs; and the reaſon of the noncommunication of the ſame electricity was evidently the impermeability of the glaſs to the electric fluid in the one caſe, and the impermeability of the air in the other. Upon this hint they made ſeveral attempts to give the electric ſhock by means of air; and at length ſucceeded, by ſuſpending large boards of wood covered with tin, with the flat ſides parallel to one another, and at ſome inches aſunder. For they found, that upon electrifying one of the boards poſitively, the other was always negative, agreeable to the former experiment: but the diſcovery was made complete and indiſputable by a perſon's touching one of the plates with one hand, and bringing his other hand to the other plate; for he then received a ſhock through his body, exactly like that of the Leyden experiment *.

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* Æpini Tentamen, p. 192.

With this plate of air, as we may call it, they made a variety of curious experiments. The two metal plates, being in oppoſite ſtates, ſtrongly attracted one another, and would have ruſhed together, if they had not been kept aſunder by ſtrings. Sometimes the electricity of both would be diſcharged by a ſtrong ſpark between them, as when a pane of glaſs burſts with too great a charge. A finger put between them promoted the diſcharge, and felt the ſhock. If an eminence was made on either of the plates, the ſelf-diſcharge would always be made through it, and a pointed body fixed upon either of them prevented their being charged at all.

The ſtate of theſe two plates, they excellently obſerve, juſtly repreſents the ſtate of the clouds and the earth during a thunder ſtorm; the clouds being always in one ſtate, and the earth in the oppoſite; while the body of air between them anſwers the ſame purpoſe as the ſmall plate of air between the boards, or the plate of glaſs between the two metallic coatings in the Leyden experiment. The phenomenon of lightning is the burſting of the plate of air by a ſpontaneous diſcharge, which is always made through eminences, and the bodies through which the diſcharge is made are violently ſhocked †.

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* Wilcke, p. 97.

† Ibid. p. 101.

THIS principle, they likewiſe thought, would explain an obſervation of the Abbé Nollet, that electricity was often obſerved to be peculiarly ſtrong, when the room was full of company, and more particularly, when numbers of them drew near together, to ſee the experiments. The conductor was then in one ſtate, and the company in another; ſo that, conſtituting a large ſurface, when any of them took a ſpark, as he thereby diſcharged the electricity of all the company, he would feel it more ſenſibly than if he had ſtood ſingle*.

THIS diſcovery, of the method of giving the electric ſhock by means of a plate of air, may be reckoned one of the greateſt diſcoveries in the ſcience of electricity ſince thoſe of Dr. Franklin. It is beautiful to obſerve how this fine diſcovery took its riſe from the experiments of Mr. Canton. Mr. Canton's experiments were purſued by Dr. Franklin, and thoſe of Dr. Franklin, purſued by theſe gentlemen, produced the diſcovery. It is one and the ſame principle that, in different circumſtances, accounts for this beautiful ſeries of experiments.

THIS experiment of charging a plate of air is likewiſe related by Mr. Æpinus, who ſays that he was led to the diſcovery, by reaſoning from the conſequences of Dr. Franklin's theory.

FROM theſe experiments he was alſo led to form a more diſtinct idea of the impermeability of glaſs to the electric fluid. For ſince a plate of air might be charged as well as a plate of glaſs, that property, whatever it be, muſt be common to them both; and could not, as Dr. Franklin once ſuppoſed, be any thing peculiar to the internal ſtructure of glaſs. Impermeability, he, therefore, infers, muſt be common to all electrics; and ſince they can all receive electricity by communication to a certain degree, it muſt conſiſt in the difficulty and ſlowneſs with which the electric fluid moves in their pores; whereas, in perfect conductors, it meets with no obſtruction at all *.

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* Wilcke, p. 96, &c.

It was chiefly this courſe of experiments, alſo, that led Mr. Æpinus to deny the exiſtence of electric atmoſpheres, conſiſting of effluvia from electrified bodies.

He ſeems, however, to conſider this as a bold opinion; ſince he herein differs, as he ſays, from all the electricians who had written before him, and even from Dr. Franklin himſelf. Though the common opinion, he ſays, is by no means countenanced by the general principles of this theory, which ſuppoſe the electric fluid to move with difficulty through every electric ſubſtance like the air.

To thoſe who might ſay, that an electric atmoſphere is a thing obvious to the ſenſes, and no matter of theory; ſince it may be felt like a ſpider's web upon the hands or face; he replies, that this feeling, together with the ſulphureous ſmell of electrified bodies, are only ſenſations excited by the action of the fluid in the electrified bodies upon the electric fluid in the noſtrils, or the hand; or upon thoſe parts of the body themſelves in an unelectrified ſtate; and that they are not felt by a perſon who is not poſſeſſed of the ſame kind and degree of electricity.

He, therefore, thinks there never was any ſufficient reaſon to admit thoſe atmoſpheres; and declares, that whenever he uſes the word, he means no more by it than the *ſphere of action* of the electricity belonging to any body. Or, he ſays, the neighbouring air, which is electrified by it, may be ſo called.

But that theſe atmoſpheres have little effect in electrical experiments, he ſays, is evident from this circumſtance; that if it be blown upon with a pair of bellows, the electricity of the body which it ſurrounds is not ſenſibly diminiſhed. The electric fluid, he ſuppoſes, to reſide wholly in the electrified body, and from thence to exert its attraction or repulſion to a certain diſtance *.

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* Æpini Tentamen, p. 82.

THE ſubject of electric atmoſpheres had not eſcaped the attention of the accurate Signior Beccaria, who was probably prior to Mr. Æpinus in ſuppoſing, that electrified bodies have no other atmoſphere than the electricity communicated to the neighbouring air, and which goes with the air, and not with the electrified bodies, agreeable to that curious diſcovery of his mentioned above.

HE alſo mentions an experiment, which, he thinks, directly proves, that all the electricity communicated to any body adheres to its ſurface, and does not ſpread into the air. He electrified a large conductor of gilt paper, in which the gilding was, in ſeveral places, taken off quite round; and obſerved that whenever he diſcharged it, by taking a ſpark at the end, other ſparks were viſible at all the interruptions; the charge of the more remote parts having come off through the ſubſtance of the metal, and not along the air; as the greateſt part of it, at leaſt, might have done, if it had lodged there †.

IT is now alſo Mr. Canton's opinion, that electric atmoſpheres are not made of effluvia from excited or electrified bodies, but that they are only an alteration of the ſtate of the electric fluid contained in, or belonging to the air ſurrounding them, to a certain diſtance; that excited glaſs, for inſtance, repels the electric fluid from it, and conſequently, beyond that diſtance makes it more denſe; whereas excited wax attracts the electric fluid exiſting in the air nearer to it, making it rarer than it was before.

THIS will be beſt underſtood by a figure. Let A (Pl. I. fig. 1.) repreſent unexcited glaſs or wax. B excited glaſs, and C

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* Æpini Tentamen, p. 257.

† Elettriciſmo artificiale, p. 54.

excited wax; and let the dots on each ſide of A repreſent a line of particles of the electric fluid at their proper diſtance in a natural ſtate.

LET B and C be carried about where you will in the air, B will make an atmoſphere equally denſe, and C an atmoſphere equally rare, while the quantity of the electric fluid each of them contains is the ſame as at firſt. When any part of a conductor comes within the atmoſphere of B, the electric fluid it naturally contains will be repelled by the denſe atmoſphere, and will recede from it. But if any part of a conductor be brought within the atmoſphere of C, the electric fluid it naturally contains will be attracted by the rare atmoſphere, and move towards it. And thus may the electric fluid contained in any body be condenſed or rarefied; and if the body be a conductor, it may be condenſed or rarefied in any part of it, and ſome may be eaſily drawn out of, or an additional quantity put into it.

IT was obſerved before, that an experiment of Dr. Franklin, which he thought proves that electric atmoſpheres did not exclude the air, might juſtly make us ſuſpect the exiſtence of thoſe atmoſpheres, ſince the electric matter is known to repel the air. Another experiment of the ſame nature was made by Dr. Darwin of Litchfield, who ſent an account of it to the Royal Society, which was read May the 5th, 1757. He got a glaſs tube, open at one end, and having a ball at the other. This ball and half of the tube he coated; and when he had inverted it, and dipped a conſiderable part of it into a veſſel containing oil of turpentine, he introduced a wire into it, and charged it; and obſerved, that the oil did not at all appear to ſubſide. From this he concluded, that the electric atmoſphere, flowing round the wire and the coating the tube, above the oil, did not diſplace the air, but exiſted in its pores *.

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* Phil. Tranſ. Vol. l. pt. i. p. 351.

AN experiment ſimilar to that of Dr. Franklin and that of Dr. Darwin was made by Signior Beccaria. He took a coated phial, and when he had inſerted into it a ſmall glaſs tube, bent horizontally when it came out of the phial; he cloſed it with cement, and preſented light aſhes to the extremity of the tube, the orifice of which was very fine; and always found, that the aſhes were blown off, when a ſpark was taken into the phial, but they returned towards the end of the tube afterwards*. It is probable, that the metal not being ſufficiently in contact with the inſide coating, a ſpark was made in the inſide, which expelled the air, and cauſed the motion in the aſhes. The faireſt method of trying it would be with a phial, in which the metal that received the fire from the conductor, ſhould be a production of the inward coating.

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* Lettere dell' elettriciſmo, p. 79.

SECTION VI.

MR. SYMMER's EXPERIMENTS RELATING TO THE TWO ELECTRICITIES, AND THOSE MADE BY JOHANNES FRANCISCUS CIGNA IN PURSUANCE OF THEM.

IT had hitherto been univerſally ſuppoſed, that all the phenomena of electricity were produced by the action of one electric fluid. Even Mr. Du Fay, at the time that he imagined he had diſcovered another electric fluid, diſtinct from that of glaſs, and peculiar to roſin, &c. thought, however, that it was quite independent of the other, and that their operations were never combined. Dr. Watſon, and Dr. Franklin thought it was very evident, that the difference between the two electricities conſiſted in the one being a redundancy, and the other a deficiency of the ſame matter. And all the experiments that had been made concerning the two electricities ſeemed to confirm this hypotheſis. At length, however, Mr. Symmer produces a great number of curious experiments, relating to the ſame ſubject; and infers from them the probable exiſtence of *two electric fluids*, not independent, but always co-exiſtent, and counteracting one another.

THE firſt ſet of his experiments are very remarkable, but he does little more than relate naked facts. They were diverſified, and purſued much farther by Mr. Cigna, of Turin, who has alſo explained them upon the principles of Dr. Franklin's theory; though he was of opinion, that no experiments that had yet been made were deciſive in favour of either of the two hypotheſes. Few hiſtories of experiments are more entertaining than the firſt of theſe of Mr. Symmer; the ſubſequent experiments are leſs ſatisfactory. The papers relating to them all were read at the Royal Society in the year 1759 *.

THIS gentleman had for ſome time obſerved, that upon putting off his ſtockings, in an evening, they made a crackling or ſnapping noiſe, and that, in the dark, he could perceive them to emit ſparks of fire. He had no doubt but that this proceeded from the principle of electricity, and, after a great number of obſervations, to determine on what circumſtances thoſe ſtrong electrical appearances depended, he found, at length, that it was the combination of white and black that produced the electricity; and that the appearances were the ſtrongeſt when he wore a white and black ſilk ſtocking upon the ſame leg †. Theſe, however, diſcovered no ſign of electricity while they were upon the leg, or hand (for he found that his hand was ſufficient) though they were drawn backwards and forwards upon it ſeveral times. Nor when taken from the hand, and preſented to an electrometer (i. e. Mr. Canton's balls) did they appear to have acquired any more than a very ſmall degree of electricity; but the moment they were ſeparated, they were found, both of them, to be highly electrified, the white poſitively, and the black negatively.

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* Phil. Tranſ. Vol. li. pt. i. p. 340.

† The Abbé Nollet, in repeating theſe experiments of Mr. Symmer, found, that it was not abſolutely neceſſary, that one of the ſtockings ſhould be black, for that, if one of them was only dipped in a decoction of gall-nuts, which doth not dye them black, but is only a preparative to it, it would have the ſame effect. Nollet's Lettres, Vol. iii. p. 42.

Both the ſtockings, when held at a diſtance from one another, appeared inflated to ſuch a degree, that, when highly electrified, they exhibited the intire ſhape of the leg; and when two black, or two white ſtockings were held together, they would repel one another, ſo as to form an angle, ſeemingly, of thirty or thirty-five degrees.

When a white and black ſtocking were preſented to each other, they would be mutually attracted; and, if permitted, would ruſh together with ſurpriſing violence. In their approach their inflation gradually ſubſided, and their attraction of foreign objects diminiſhed, but their attraction of one another increaſed. When they actually met, they grew flat, and joined as cloſe together, as if they had been ſo many folds of ſilk. When they were ſeparated, their electricity did not ſeem to have been in the leaſt impaired by the ſhock of meeting; for they would be again inflated, attract, repel, and ruſh together as before.

When this experiment was performed with two black ſtockings in one hand, and two white ones in the other, it exhibited a curious ſpectacle. The repulſion of thoſe of the ſame colour and the attraction of thoſe of different colours, threw them into an agitation which was not unentertaining, and made them catch each at the oppoſite colour, at a greater diſtance than could have been expected.

When the ſtockings were ſeparated from one another, they would loſe their power very ſoon, much like the excited tube; but when they were together, they would retain it an hour or two, or longer, if the air was favourable to electricity. The ſharpeſt metallic point could not deprive them of it; and when they were one within the other, no means he could think of could procure the leaſt perceivable diſcharge of the electricity. In this reſpect, Mr. Symmer thought there was a conſiderable reſemblance between the black and the white ſtocking, when put within one another, and the Leyden phial.

WHAT was ſtill more remarkable in theſe experiments with the white and black ſtockings, was the power of electrical coheſion which they exhibited. Mr. Symmer perceived that the white and black ſtockings, when electrified, and allowed to come together, not only joined extremely cloſe, but actually ſtuck to each other. By means of a balance, he found, that in order to ſeparate them, it required from one to twelve ounces. Another time they raiſed ſeventeen ounces, which was twenty times the weight of that ſtocking which ſupported them, and this in a direction parallel to its ſurface.

WHEN one of the ſtockings was turned inſide out, and put within the other, it required twenty ounces to ſeparate them, though when they were applied to each other externally, ten ounces were ſufficient.

GETTING the black ſtockings new dyed, and the white ones waſhed, and whitened in the fumes of ſulphur; and then putting them one within the other, with their rough ſides together, it required three pounds three ounces to ſeparate them. And he had reaſon to think that the ſulphur contributed nothing to the experiment.

TRYING this experiment with ſtockings of a more ſubſtantial make, he found the effects more conſiderable. When the white ſtocking was put within the black one, ſo that the outſide of the white was contiguous to the inſide of the black, they raiſed nine pounds wanting a few ounces, which was fifty times the weight of the ſtocking. When the white ſtocking was turned inſide out, and put within the black one, ſo that their rough ſurfaces were contiguous, they raiſed fifteen pounds one pennyweight and a half, which was ninety-two times the weight of the ſtocking.

Having cut off the ends of the thread, and the tufts of ſilk, which had been left in the inſide of the ſtockings, the coheſion was conſiderably diminiſhed. Preſſing them together between his hands contributed much to ſtrengthen it*.

When the white and black ſtocking were in coheſion, and another pair, more highly electrified were ſeparated from one another, and preſented to the former, their coheſion would be diſſolved; and each ſtocking of the ſecond pair would catch hold of, and carry away with it, that of its oppoſite colour. If the degree of electricity of both pairs were equal, the coheſion of the former pair would be weakened, but not diſſolved; and all the four would cohere, forming one maſs. If the ſecond pair were but weakly electrified, the coheſion of the firſt pair would be but little impaired, and the coheſion of the whole maſs would be ſmall in proportion.

Mr. Symmer alſo obſerved, that white and black ſilk, when electrified, not only cohered with each other, but would alſo adhere to bodies with broad, and even with poliſhed ſurfaces, though thoſe bodies were not electrified. This he diſcovered accidentally, having, without deſign, thrown a ſtocking out of his hand, which ſtuck to the paper hangings of the room. He repeated the experiment, and found it would continue hanging near an hour.

Having ſtuck up the black and white ſtockings in this manner, he came with another pair of ſtockings highly electrified; and applying the white to the black, and the black to the white, he carried them off from the wall, each of them hanging to that which had been brought to it.

The ſame experiments held with the painted boards of the room, and likewiſe with the looking-glaſs, to the ſmooth ſurface of which both the white and the black ſilk appeared to adhere more tenaciouſly than to either of the former [*].

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* Phil. Tranſ. Vol. li. pt. i. p. 393.

A FEW obſervations, ſimilar to ſome of theſe of Mr. Symmer, were made by Signior *Aleſſandro Amadeo Vaudonia*, a friend of Signior Beccaria. He put a beaver ſhirt between two others, which he wore in extreme cold weather; and whenever he put off the uppermoſt ſhirt, which he did every day, he found it adhered to the beaver ſhirt, and, on the ſeparation, electric ſparks were viſible between them. Whenever he put off the beaver ſhirt, it adhered ſtill more to the under ſhirt, and when held at a conſiderable diſtance from it, would ruſh to it. Theſe attractions would be repeated many times, but they grew more languid by degrees, till they intirely ceaſed. Signior Beccaria, upon hearing of this experiment, repeated it with ſome variation, and found it to anſwer on himſelf [†].

THE coheſion of the two ſtockings induced Mr. Symmer to try the force of electrical coheſion in electrified panes of glaſs. For this purpoſe, he got two panes of common window glaſs, the thinneſt and the ſmootheſt that he could find, and coated one of the ſides of each with tinfoil, leaving a ſpace uncovered near the edges. He then put the uncovered ſides together, and charging them both as one pane, he found, as he expected, that their coheſion was conſiderably ſtrong: but he had no apparatus to meaſure the ſtrength of it. He then turned the plates upſide down, and found that the ſame operation which had before charged them, did now uncharge them, according to the analogy of the Leyden phial.

PLACING two panes of glaſs, each of them coated on both ſides, one upon the other, he found that they were both charged ſeparately, and that there was no coheſion between them.

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[*] Phil. Tranſ. Vol. li. pt. i. p. 366.

[†] Dell' elettriciſmo artificiale, p. 197.

In purſuance of theſe laſt mentioned experiments of Mr. Symmer, and another made at Pekin (which will be recited preſently) Signior Beccaria made the following, which are very curious; but which, after the example of the author, I ſhall relate without attempting an explanation.

Having charged a coated plate of glaſs, he ſlipped the coating from off the negative ſide, and applied another uncoated and uncharged plate of glaſs cloſe to it. After this, putting a coating upon the uncharged glaſs (ſo that the whole reſembled one coated plate, conſiſting of two *laminæ)* he formed a communication between the coatings. The conſequence was an exploſion, and a coheſion of the plates.

If he ſeparated the plates before the exploſion, after they had been in conjunction ſome time, the charged plate was poſitive on both ſides, and the uncharged plate negative on both ſides. But if he ſeparated them after the exploſion, the charged plate was negative on both ſides, and the uncharged plate poſitive on both ſides.

If after the exploſion he ſeparated and joined them again alternately, a ſmall circle of paper, placed under the uncharged plate, adhered to it upon every ſeparation, and was thrown off again upon every conjunction. This he could repeat even five hundred times, with once charging the plate. This was the experiment that he ſays was made by ſome Jeſuits at Pekin, in the year 1755, and being ſent to the Academy of Sciences at Peterſburgh, was publiſhed in their Memoirs, vol. viii. p. 276.

If, in theſe experiments, the charged plate was inverted, and the poſitive ſide applied to the uncharged plate, all the effects were exactly the reverſe of the former. If it was inverted ever ſo often, after remaining ſome time in contact with the uncharged glaſs, it would produce a change in the electricity. In the dark, a light was always ſeen upon the ſeparation of theſe plates.

Laying the two plates together, like one plate, and coating the outſides of them, he charged them both together, and, at the diſtance of about four feet, he diſtinguiſhed ſix of the *coloured rings*, which Newton deſcribes in his book of Optics, all parallel to one another, and nearly parallel to the edge of the coating. At the angles of the coatings the rings ſpread to a greater diſtance; where the coatings did not quite touch the glaſs, the rings bent inwards; and where the coatings adhered very cloſe, they retired farther from them. Upon diſcharging theſe two plates, the coloured rings vaniſhed, and the electric coheſion (which Mr. Symmer had obſerved in this caſe) ceaſed with them.

Upon ſeparating theſe plates before the exploſion, that which had received the poſitive electricity was poſitive on both ſides, and the other negative on both ſides. If they were ſeparated after the exploſion, each of them (as in the former experiment) was affected in a manner juſt the reverſe of this. Upon inverting theſe plates, that which was the thinner appeared to be poſſeſſed of the ſtronger electricity, and (like the charged plate in the former experiment) brought the other to correſpond to it.

Charging the two plates ſeparately, and taking off two of the coatings, ſo as to place the two poſitive or the two negative ſides together, there was no coheſion or exploſion. But joining a poſitive and a negative ſide, they cohered, and a communication being formed on the outſide, there was an exploſion, which increaſed the coheſion. Making the above-mentioned experiments with theſe plates, he ſays, they acted juſt as the two that were charged at the ſame time*.

Mr. Symmer concludes his account of theſe experiments with declaring it to be his opinion, that there are two electric fluids, or emanations of two diſtinct electric powers, eſſentially different from each other; that electricity does not conſiſt in the afflux and efflux of theſe fluids, but in the accumulation of the one or the other of them in bodies electrified; or, in other words, it conſiſts in the poſſeſſion of a larger portion of one or the other power, than is requiſite to maintain an even balance within the body; and laſtly, that, according as the one or the other power prevails, the body is electrified in the one or the other manner. Nor will this principle, ſays he, of two diſtinct electrical powers be found, upon due conſideration, to diſagree with the general ſyſtem of nature. It is one of the fundamental laws of nature, that action and reaction are inſeparable and equal; and, when we look round, we find that every power which is exerted in the material world meets with a counteracting power, which controuls and regulates its effects, ſo as to anſwer the wiſe purpoſes of providence *.

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* Phil. Tranſ. Vol. lvii. p. 458.

MR. SYMMER alſo alledges, in proof of his two diſtinct powers of electricity, the experiment which Dr. Franklin has related, of piercing a quire of paper with an electric ſhock. He thought that the bur which was raiſed on both ſides of the paper was produced by two fluids, moving in two different directions. To ſhow the manner in which this ſtroke was made more evidently, he mentions two other ſimilar experiments, in which the circumſtances of the ſtroke were a little varied.

A PIECE of paper, covered on one ſide with Dutch gilding, and which had been left accidentally between two leaves, in a quire of paper in which the former experiment had been made, was found to have the impreſſion of two ſtrokes upon it, about a quarter of an inch from each other; the gilding being ſtripped off, and the paper left bare for a little ſpace in both places. In the center of one of theſe places was a little round hole, in the

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* Phil. Tranſ. Vol. li. pt. i. p. 389.

the other only an indenture or impreſſion, ſuch as might have been made with the point of a bodkin.

THESE obſervations Mr. Symmer communicated to Dr. Franklin, who, notwithſtanding Mr. Symmer was endeavouring to eſtabliſh a theory of electricity contrary to his own, with the generoſity natural to him, aſſiſted him with his apparatus in making another experiment in purſuance of that mentioned above.

IN the middle of a paper book, of the thickneſs of a quire, Mr. Symmer put a ſlip of tinfoil; and in another, of the ſame thickneſs, he put two ſlips of the ſame ſort of foil, including the two middle leaves of the book between them. Upon ſtriking the two different books, the effects were anſwerable to what he expected. In the firſt, the leaves on each ſide of the foil were pierced, while the foil itſelf remained unpierced; but, at the ſame time, he could perceive an impreſſion had been made on each of its ſurfaces, at a little diſtance from one another; and ſuch impreſſions were ſtill more viſible on the paper, and might be traced, as pointing different ways. In the ſecond, all the leaves of the book were pierced, excepting the two that were between the ſlips of foil; and in theſe two, inſtead of holes, the two impreſſions in contrary directions were viſible.

MR. SYMMER afterwards got an electrical apparatus of his own, formed on the model of that of Dr. Franklin, with which he frequently repeated the experiments above-mentioned, the reſult of all which he compriſes in the three following obſervations.

1. WHEN a quire of paper, without any thing between the leaves, is pierced with a ſtroke of electricity, the two different powers keep in the ſame track, and make but one hole in their paſſage through the paper: not but that the power from above, or that from below, ſometimes darts into the paper at two or more different points, making ſo many holes, which, however, generally unite before they go through the paper. They ſeem to paſs

each other about the middle of the quire, for there the edges are moſt viſibly bent different ways; whereas, in the leaves near the outſide of the quire, the holes very often carry more the appearance of the paſſage of a power iſſuing out, and exploding into the air, than of one darting into the paper.

2. When any thin metallic ſubſtance, ſuch as gilt leaf, or tinfoil, is put between the leaves of the quire, and the whole is ſtruck, in that caſe the counteracting powers deviate from the direct tract, and leaving the path which they would in common have taken through the paper, only make their way in different lines to the metallic body, and ſtrike it in two different points, diſtant from one another about a quarter of an inch, more or leſs; the diſtance appearing to be the leaſt when the power is greateſt: and whether they pierce it, or only make impreſſions upon it, in either caſe they leave evident marks of motion from two different parts, and in two contrary directions. It is this deviation from a common courſe, and the ſeparation of the lines of direction conſequent upon it, ſays he, that affords a proof of the exertion of two diſtinct and counteracting powers.

3. When two ſlips of tinfoil are put into the middle of the quire, including two or more leaves between them, if the electricity be moderately ſtrong, the counteracting powers only ſtrike againſt the ſlips, and leave their impreſſion there. When it is ſtronger, one of the ſlips is generally pierced, but ſeldom both; and from what he had obſerved in ſuch caſes, he ſays it ſhould ſeem, as if the power which iſſued from the outſide of the phial acted more ſtrongly than that which proceeded from within, for the lower ſlip was moſt commonly pierced. But this, he adds, may be owing to the greater ſpace which the power from within has to move through before it ſtrikes the paper *.

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* Phil. Tranſ. Vol. li. pt. i. p. 377, &c.

In the ſame paper, Mr. Symmer furniſhes a remarkable inſtance of the power of an hypotheſis in drawing facts to itſelf, in making proofs out of facts which are very ambiguous, and in making a perſon overlook thoſe circumſtances in an experiment which are unfavourable to his views.

When a phial is electrified but a little, Mr. Symmer ſays, if we touch the coating of it with a finger of one hand, and, at the ſame time, bring a finger of the other hand to the wire, we ſhall receive a pretty ſmart blow upon the tip of each of the fingers, the ſenſation of which reaches no farther. If the phial be electrified a degree higher, we ſhall feel a ſtronger blow, reaching to the wriſts, but no farther. When, again, it is electrified to a ſtill higher degree, a ſeverer blow will be received, but will not be felt beyond the elbows. Laſtly, when the phial is ſtrongly charged, the ſtroke may be perceived in the wriſts, and elbows; but the principal ſhock is felt in the breaſt, as if a blow from each ſide met there. This plain and ſimple experiment, ſays Mr. Symmer, ſeems obviouſly to ſuggeſt to obſervation the exiſtence of two diſtinct powers, acting in contrary directions; and, I believe, ſays he, it would be held as a ſufficient proof by any perſon, who ſhould try the experiment, with a view to determine the queſtion ſimply from his own perceptions *.

It is a ſufficient anſwer to this remark of Mr. Symmer, that if twenty people join hands, they may all be made to feel the ſhock in their wriſts, or their elbows, without having their breaſts affected in the leaſt. And can it be ſuppoſed, that the two currents of electric fire could come at all their wriſts or elbows, without paſſing through their breaſts? According to Mr. Symmer's hypotheſis, it ſhould ſeem, that, in a large circle, thoſe

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* Phil. Tranſ. Vol. li. pt. i. p. 373, &c.

perſons only who ſtood near the phial, on either hand, ſhould feel *a ſmall ſhock*; that a few perſons more, at each extremity of the circle, ſhould feel one ſomething ſtronger; and that it could only be a very ſtrong ſhock, which could at all affect the perſon who ſtood in the middle; and that then he ſhould be affected the leaſt of any perſon in the company. But all theſe conſequences are contrary to fact.

THIS hypotheſis of Mr. Symmer, notwithſtanding he has failed in his application of it to the experiments above mentioned, has attracted the notice of ſeveral electricians, both at home and abroad; and ſome perſons ſeem inclined to adopt it, in preference to Dr. Franklin's theory. I ſhall therefore conſider it more at large, when I come to treat of *theories* profeſſedly; till which time, I take leave of this ingenious philoſopher, and his two electric fluids.

THE experiments of Mr. Symmer excited the attention of Mr. Cigna, and led him to a courſe of experiments, which throws ſtill more light, both upon the doctrine of the two electricities and the Leyden phial. They are alſo a farther illuſtration of the diſcovery of Mr. Canton, improved by Meſſrs. Wilcke and Æpinus, of the mutual repellency of ſimilar electric atmoſpheres.

HE took two white ſilk ribbons, juſt dried at the fire, and having extended them upon a ſmooth plain, either a conductor or a non-conductor, he drew over them the ſharp edge of an ivory ruler, and found, that both the ribbons had acquired electricity enough to adhere to the plain; though, while they remained upon the plain, they ſhewed no other ſign of it. If they were both taken off from the plain together, they attracted one another, the upper having acquired the reſinous and more powerful, and the lower the vitreous and weaker electricity. If they were taken up ſeparately, they repelled one another, having both acquired the reſinous electricity *.

In this ſeparation of both the ribbons from the plain, as alſo in their ſeparation, afterwards, from one another, electric ſparks were viſible between them; but if they were again put upon the plain, or joined together, no light appeared upon their ſecond ſeparation, without another friction of the ruler. Alſo, when, by being taken off ſeparately, they had been made to repel one another, if they were laid on the plain again, and taken off together, they would not attract; and if, by being taken off together, they had firſt been made to attract one another, and were laid on the plain a ſecond time, and then taken off ſeparately, they would not repel, without another friction.

When, by the operation above mentioned, they had acquired the ſame electricity, if they were placed, not upon the ſmooth body on which they had been rubbed, but on a rough one, and a conductor, as hemp or cotton, not very dry; they would, upon being ſeparated, ſhow contrary electricities; which, when they were joined together, would diſappear as before †

If they had been made to repel one another, and were afterwards placed one upon the other, on the rough urface above mentioned, they would, in a few minutes, attract one another; the lower of the two ribbons having changed its reſinous into a vitreous electricity.

If the two white ribbons received their friction upon the rough ſurface, they always acquired contrary electricities; the upper of the two having the reſinous, and the lower the vitreous, in whatever manner they were taken off.

The ſame thing that was done by a rough ſurface was done by any pointed conductor. If two ribbons, for inſtance, were made

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* Memoirs of the Academy at Turin for the year 1765, p. 31.

† Ibid. p. 33.

made to repel, and hang parallel to one another; and the point of a needle were drawn oppoſite to one of them, along its whole length, they would preſently ruſh together; the electricity of that ribbon to which the needle was preſented being changed into the contrary *.

In the ſame manner in which one of the ribbons changed its electricity, a ribbon not electrified would acquire electricity, viz. by putting it upon a rough ſurface, and laying an electrified ribbon upon it; or by holding it parallel to an electrified ribbon, and preſenting a pointed conductor to it.

He placed a ribbon not quite dry under another that was well dried at the fire, upon a ſmooth plain; and when he had given them the uſual friction with his ruler, he found that, in what manner ſoever they were removed from the plain, the upper of them had acquired the reſinous, and the lower the vitreous electricity †.

If both the ribbons were black, all the above mentioned experiments ſucceeded, in the ſame manner as if they had been white ‡.

If, inſtead of his ivory ruler, he made uſe of any ſkin, or of a piece of ſmooth glaſs, the event was the ſame; but if he made uſe of a ſtick of ſulphur, the electricities were, in all caſes, the reverſe of what they were before; the ribbon which was rubbed having always acquired the vitreous electricity.

When he made uſe of paper, either gilt or not gilt, the reſults were uncertain.

When the ribbons were wrapped in paper, gilt or not gilt, and the friction was made upon the paper, laid upon the plain above mentioned, the ribbons acquired, both of them, the reſinous electricity §.

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* Memoirs of the Academy at Turin for the year 1765, p. 34.

† Ibid. p. 35.

‡ Ibid.

§ Ibid. p. 36.

If the ribbons were one black, and the other white, which ever of them was laid uppermoſt, and in whatever manner the friction was made, the black generally acquired the reſinous, and the white the vitreous electricity [*].

He obſerved, however, the following conſtant event; that whenever the texture of the upper piece of ſilk was looſe, yielding, and retiform, like that of a ſtocking, ſo that it could move, and be rubbed againſt the lower, and the rubber was of ſuch a nature as to impart but little electricity to glaſs; the electricity which the upper piece of ſilk acquired, did not depend upon the rubber, but upon the body it was laid upon; in which caſe the black was always reſinous, and the white vitreous. But when the ſilk was of a cloſe texture, hard and rigid, and when the rubber was ſuch as imparted a great degree of electricity to glaſs, the electricity of the upper piece did not depend upon the lower, but upon the rubber. Thus a white ſilk ſtocking, rubbed with gilt paper upon glaſs, became reſinous, and the glaſs vitreous; but if a piece of ſilk, of a firmer texture, was laid upon a plate of glaſs, it always acquired the vitreous electricity, and the glaſs the reſinous, if it was rubbed with ſulphur; and for the moſt part, if it was rubbed with gilt paper [†]. So that the ſilk that was rubbed received its electricity, ſometimes from the rubber, and ſometimes from the ſubſtance placed under it; according as it received greater friction from the one or the other, or in proportion as one or the other was more proper to give electricity to glaſs.

Another ſet of experiments, which the ſame Mr. Cigna made, illuſtrate the adheſion of Mr. Symmer's electrical ſtockings to bodies with ſmooth ſurfaces. He inſulated a plate of lead, and bringing an electrified ribbon near it, obſerved that it was attracted very feebly. Bringing his finger to the lead, a ſpark iſſued out of it, upon which it attracted the ribbon vigorouſly, and both together ſhewed no ſigns of electricity. Upon the ſeparation of the ribbon, they again both appeared to be electrified, and a ſpark was perceived between the plate and the finger *.

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[*] Memoirs of the Academy at Turin for the year 1765, p. 38.

[†] Ibid. p. 40.

Laying two plates of glaſs upon a ſmooth conductor communicating with the ground, and rubbing them in the ſame manner as the ribbons had been rubbed, they likewiſe acquired electricity, and adhered firmly, both to one another, and to the conductor. If it were a plate of lead, not very thick, it would be ſupported by the attraction. When they were together, they ſhowed no other ſigns of electricity †.

When the two plates of glaſs were ſeparated from the conductor, and kept together, they ſhowed, on both ſides, a vitreous electricity; and the conductor, if it had been inſulated, was ſeen to have contracted a reſinous electricity.

The two plates of glaſs themſelves, when ſeparated, were poſſeſſed of the two electricities; the upper of the vitreous and ſtronger, and the lower of the reſinous and weaker.

With a rough conductor, whether they were originally rubbed upon it, or brought to it, after they had been rubbed upon a ſmooth one, they ſcarce contracted any electricity; though, when they were ſeparated from one another, they were affected as before.

Upon this principle, Mr. Cigna endeavours to account for the non-excitation of a globe or tube from which the air is exhauſted, or which is lined with conducting ſubſtances. In this caſe, he ſays, the vitreous electricity on the external ſurface of the glaſs is balanced by the reſinous in the inward coating, or in the

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* Memoirs of the Academy at Turin, for the year 1765, p. 43.

† Ibid. p. 52.

vacuum which ſerves inſtead of a coating; and therefore it is in the ſituation of the plates of glaſs while they lie upon the conductor above-mentioned: but when the inward coating is taken away, the electricity appears on the outſide, without any freſh excitation, as when the plates were removed from the conductor *.

WHEN he laid a number of ribbons of the ſame colour upon the ſmooth conductor, and drew his ruler over them; he found, that when he took them up ſingly, they all gave ſparks, at the place where they were ſeparated, as the laſt ribbon did with the ſmooth plate, and had all acquired the reſinous electricity †.

IF they were all taken from the plate together, they cohered in one maſs, which, on both ſides, appeared to be reſinous. If they were laid upon the rough conductor, in the ſame order (whereby the oppoſite electricities were brought to an equilibrium) and they were all ſeparated ſingly, beginning with the loweſt, ſparks appeared as before; but all the ribbons had acquired the vitreous electricity, except the uppermoſt, which retained the reſinous electricity it had received from the friction ‡.

IF they received the friction upon the rough conductor, and were all taken up at once (in order to have a bundle in which the oppoſite electricities were balanced) all the intermediate ribbons acquired the electricity, either of the higheſt or the loweſt ribbon, according as the ſeparation was begun with the higheſt, or the loweſt.

IF two ribbons were ſeparated from the bundle at the ſame time, they clung together; and, in that ſtate, ſhowed no ſign of electricity, as one of them alone would have done. When they were ſeparated, and the different electricities were manifeſt, the electricity was obſerved to reſide in the outermoſt, and was op-

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* Memoirs of the Academy at Turin, for the year 1765, p. 54.

† Ibid. p. 61. ‡ Ibid.

poſite to that by which they had both adhered to the bundle, but much weaker [*].

HE placed a number of ribbons upon a plate of metal, which received electricity from the globe, while he held a pointed body to the other ſide of the ribbons. The conſequence was, that all the ribbons became poſſeſſed of the electricity oppoſite to that of the plate, or of the ſame, according as they were taken off; except the moſt remote, which always kept an electricity oppoſite to that of the plate.

FROM theſe experiments he infers, that as electricity is propagated from the outermoſt ribbon to thoſe underneath it, or elſe from the plate below to thoſe next above it, when they are ſeparated, ſo likewiſe when the coating is ſeparated from a charged pane of glaſs, it likewiſe depoſits its electricity upon the ſuperficies of the glaſs, the phenomena being the ſame in both. For when he put metal coatings on the ſide of a plate of glaſs, without any cement, they adhered firmly to the glaſs when it was charged, and a light appeared upon their being ſeparated from it, as in the caſe of the ribbons [†].

WHEN he coated a number of ribbons in the ſame manner, and charged them, the coatings adhered firmly to the ribbons; but he could never ſeparate one of them, but (in conſequence of the looſe texture of the ſilk) a ſpark would go to the oppoſite coating, which immediately fell off, the whole being then diſcharged [‡].

BUT he thought the coatings did not depoſite all their electricity on the plate, when they were taken off; for though, when both were taken off, the electricities of the two ſides ſtill balanced one another (becauſe each retained the ſame diminiſhed quantity) yet, when one ſuperficies of the glaſs, or of the ribbons, received its electricity from friction, and the other only from the oppoſite coating, he obſerved, that the electricities which balanced one another while the coating was on, were no longer balanced when it was taken off; the electricity of the ſurface which was rubbed then prevailing, becauſe the conducting coating had, upon its ſeparation, taken part of its electricity along with it [*].

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[*] Memoirs of the Academy at Turin, for the year 1765, p. 61.

[†] Ibid. p. 63. [‡] Ibid. p. 64.

To confirm this, he adds another experiment. He charged a pane of glaſs, coated on one ſide, while the other received electricity by a pointed conductor from the machine: he likewiſe inverted the plate, and made the coated ſide communicate with the prime conductor, while a pointed piece of metal was preſented to the oppoſite ſide; and, in both caſes, found, that while the coating remained, the two electricities balanced one another; but that when the coating was ſlipped off, the electricity of the oppoſite ſide prevailed, ſo as to be apparent on both ſides of the plate [†].

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[*] Memoirs of the Academy at Turin for the year 1765, p. 65.

[†] Ibid.

SECTION VII.

THE HISTORY OF THE LEYDEN PHIAL CONTINUED.

GREAT as were the diſcoveries of Dr. Franklin concerning the Leyden phial, he left ſome curious particulars for this period of the hiſtory of electricity; and the ſubject is by no means exhauſted. Many of the properties of this *wonderful bottle*, as the Doctor calls it, are ſtill unexplained. But as more and more light is perpetually thrown upon it, let us hope that, at length, we ſhall thoroughly underſtand this great experiment. The greateſt diſcovery concerning the properties of the Leyden phial, in this period, hath already been related in the account of Meſſrs. Wilcke's and Æpinus's method of giving the ſhock by means of a plate of air; and other obſervations have, likewiſe, been occaſionally mentioned, in places where their connection required them to be inſerted. This ſection, however, will contain ſeveral experiments of a miſcellaneous nature, which are well worth notice.

IMMEDIATELY upon the diſcovery of the ſhock given by glaſs, all electricians attempted to charge other electric ſubſtances, but none of them ſucceeded before Signior Beccaria. He found that a very ſmooth plate of ſealing-wax, made by pouring that ſubſtance, when melted, upon an oiled marble table, would receive a conſiderable charge *.

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* Lettere dell' elettriciſmo, p. 64.

AFTER trying ſeveral other electrics, in the ſame manner, he found that a mixture of pitch and colophonia was charged leſs than ſealing-wax, but more than ſulphur, and a great deal more than pitch alone *.

BUT the moſt curious experiment of this philoſopher, relating to this ſubject, was made with a view to aſcertain the real direction of the electric fluid in a diſcharge. He ſuſpended a coated plate of glaſs by a ſilk thread, and having charged it, and kept it perfectly ſtill; he obſerved that no motion was given to it, when the diſcharge was made by a crooked wire approaching both the ſides at the ſame time. The experiment, in fact, proved the re-action of the glaſs upon the electric matter; whereby the plate were kept ſtill, notwithſtanding the fluid ruſhed with great violence from one ſide to the other. He compares the glaſs to an ivory ball placed between two others, which keeps its place, when, by an impulſe given to one of them, the oppoſite ball flies off †.

MR. HARTMAN of Hanover has publiſhed an account of a curious experiment, which ſeems to ſhow the progreſſive motion of the electrical exploſion. He paſſes a ſhock through a great number of cannon balls, ſometimes to the number of forty, placed near one another, upon ſmall drinking-glaſſes; when all the ſparks are ſeen at the ſame moment of time, and all the ſnappings make but one report. But when he ſubſtitutes eggs inſtead of the balls of metal, the progreſs of the exploſion is viſible, every two giving a ſnap and a flaſh ſeparately. This experiment requires weather very favourable to electricity, and, he ſays has generally ſucceeded the beſt with only ten or twelve eggs ‡. This author has not expreſsly ſaid in which direction the ſparks ran; but as he adopts Dr. Franklin's hypotheſis, it may be

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* Lettere dell' elettriciſmo, p. 66.

† Ibid. p 72.

‡ Abhandlung, p. 58, &c.

preſumed, that he imagined them to go from the poſitive to the negative ſide of the charged glaſs.

A very ingenious experiment has alſo been made by Mr. Amadeus Lullin of Geneva, in order to aſcertain the direction of the electric fluid in exploſions, and which he thinks comes nearer to the *experimentum crucis* than any other. He placed a common card in the circuit of the electrical exploſion, while the wire which communicated with the poſitive ſide of the jar lay on one ſide of it, and that which communicated with the negative ſide lay on the other, their extremities not being placed oppoſite to one another, but at ſome diſtance. Things being thus circumſtanced, he obſerved, that upon making the diſcharge, the card was conſtantly pierced cloſe to the extremity of the wire which communicated with the negative ſide of the jar, as if the electric fluid, ruſhing from the poſitive ſide of the jar into the wire which communicated with it, and iſſuing from the extremity of it, was driven by its own impulſe along the ſurface of the card, and did not pierce it, till it came oppoſite to the extremity of the other wire, which communicated with the negative ſide, by which it was ſtrongly attracted *.

A very curious and elegant experiment on the Leyden phial was made by profeſſor Richman of Peterſburgh, whoſe unfortunate death will be related in this hiſtory.

He coated both ſides of a pane of glaſs, within two or three inches of the edge, and faſtened linen threads to the upper part of the coating, on both ſides; which, when the plate was not charged, hung down in contact with the coating; but ſetting the plate upright, and charging it, he obſerved, that when neither of the ſides was touched by his finger, or any other conductor communicating with the earth, both the threads were repelled from the coating, and ſtood at an equal diſtance from it; but when he brought his finger, or any other conductor, to one of the ſides, the thread hanging to that ſide fell nearer to the coating, while the thread on the oppoſite ſide receded as much; and that when his finger was brought into contact with one of the ſides, the thread on that ſide fell into contact with it likewiſe, while the thread on the oppoſite ſide receded to twice the diſtance at which it hung originally; ſo that the two threads always hung ſo as to make the ſame angle with one another [*].

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* Diſſertatio Phyſica, p. 24.

ÆPINUS ſhows, that it is not ſtrictly true, that an inſulated perſon, diſcharging the Leyden phial through his own body, contracts no electricity. Electrifying a large plate of air, he obſerved, that if the nearer plate, (by which I ſuppoſe he means that which he firſt touched) was electrified poſitively, he acquired a poſitive electricity by the diſcharge; but if it were negative, he acquired a negative electricity. He ſuppoſes that the reaſon why the experiment did not ſucceed with Dr. Franklin, was, that the ſurfaces with which he tried the experiment were not large enough to make the effect ſenſible; and that the diſtance of the metal plates was, likewiſe, too ſmall, as it neceſſarily muſt be in charging of glaſs [†].

MR. CIGNA has invented a new method of charging a phial, upon the principle diſcovered by Mr. Canton and Mr. Wilcke, viz. that the electricity of one body repels that of another, eſpecially if it have a flat ſurface, and gives it the contrary electricity.

HE inſulates a ſmooth plate of lead, and while he brings an electrified body, as a ſtocking, to it, he takes a ſpark with the wire of a phial from the oppoſite ſide; and removing the ſtocking, he takes another ſpark with his finger, or any conductor communicating with the ground. Bringing the ſtocking

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[*] Æpini Tentamen, p. 335.

[†] Ibid. p. 27.

ing nearer the plate a ſecond time, he takes a ſecond ſpark, with the wire of the phial, as before; and, removing it again, takes another, in the ſame manner, with his finger. This operation he continues, till the phial is charged; which, in favourable weather, may be done with very little diminution of the electricity of the ſtocking *.

If, inſtead of taking a ſecond ſpark with his finger, he had taken it with the wire of another phial, that would have been charged likewiſe, with no additional labour, and with an electricity oppoſite to that of the other phial. If the ſecond ſpark were taken with the coating of the ſame phial, the charging would be accelerated, but the operation would be troubleſome to manage.

The theory of this new method of charging a phial is very eaſy, upon the principle referred to above. The approach of the electricity in the ſtocking, not being able to enter the broad ſmooth ſurface of the metal, drives the electric fluid out of that part of the plate which is oppoſite to it, to the other ſide, which, being thereby overcharged, will part with its ſuperfluity to the wire of the phial. The ſtocking being taken away, the plate will have leſs than its natural ſhare of the electric fluid, and will therefore readily take a ſpark, either from the finger, or the wire of another phial †.

The ſame ingenious philoſopher makes a conſiderable difference between the electric fluid which gives the ſhock, and that on which ſome other phenomena of coated glaſs depend. The former, which is far the greateſt quantity, he ſuppoſes to reſide, either in the coating itſelf, or on the ſurface of the glaſs; whereas the other, he imagines, to have entered the pores, and affected the ſubſtance of the glaſs itſelf.

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* Memoirs of the Academy at Turin for the year 1765, p. 49.

† Ibid. p. 51.

He laid two plates of glaſs, well dried, one upon the other, as one piece; the lower of them being coated on the outſide; and, when they were inſulated, he alternately rubbed the uppermoſt plate with one hand, and took a ſpark from the coating of the lower with the other, till they were charged; when the coating, and both the plates adhered firmly together. Giving a coating to the other ſide, and making a communication between that and the other coating, the uſual exploſion was made. But the plates, though thus diſcharged, ſtill cohered; and though, while they were in this ſtate, they ſhowed no other ſign of electricity; yet, when they were ſeparated, they were each of them found to be poſſeſſed of an electricity oppoſite to that of the other.

If the two plates were ſeparated before they were diſcharged, and the coating of each were touched, a ſpark came from each; and when put together, they would cohere as before, but were incapacitated for giving a ſhock *.

He, therefore, compares the electricity which gives the ſhock to the electricity of the metal plate in the former experiment; which is loſt with taking one ſpark, as the ſilk is removed from it, and is different from the electricity by which the two plates of glaſs cohere. The one is diſperſed at once, but the other ſlowly; the one exiſting, as he ſuppoſes, in the conductors, or upon the ſurfaces of the electrics, and the other in the ſubſtance itſelf †.

Among experiments relating to the electric ſhock, we ought to mention what has been obſerved within this period of its amazing force in melting wires, and producing other ſurpriſing effects.

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* Memoirs of the Academy at Turin for the year 1765, p. 55.

† Ibid. p. 56.

That even artificial electricity, ſays Dr. Watſon, in a paper read at the Royal Society June 28th, 1764, when in too great a quantity, and hurried on too faſt, through a fine iron wire, has a remarkable effect upon the wire, appears from a very curious experiment of Mr. Kinnerſley. This gentleman, in the preſence of Dr. Franklin, made a large caſe of bottles explode at once through a fine iron wire. The wire at firſt appeared red-hot, and then fell into drops, which burned themſelves into the ſurface of his table or floor. Theſe drops cooled in a ſpherical figure like very ſmall ſhot, of which Dr. Franklin tranſmitted ſome to Mr. Canton, who repeated the experiment. This proves the fuſion to have been very complete; as nothing leſs than the moſt perfect fluidity could give this figure to melted iron.

Mr. Canton, in a note ſubſcribed to the ſame paper, obſerves, that the diameter of a piece of Mr. Kinnerſley's wire, which he received from Dr. Franklin, was one part in 182 of an inch. He adds, that artificial lightning, from a caſe of thirty-five bottles, would entirely deſtroy braſs wire, of one part in 330 of an inch. At the time of the ſtroke, he ſays, a great number of ſparks, like thoſe from a flint and ſteel, would fly upwards, and laterally, from the place where the wire was laid, and loſe their light, in the day-time, at the diſtance of about two or three inches. After the exploſion, a mark appeared on the table, the whole length of the wire, and ſome very round particles of braſs were diſcovered by a magnifier near the mark, but no part of the wire itſelf could be found *.

Signior Beccaria was able to melt ſmall ſlips of metal, without incloſing, or covering them with pieces of glaſs. But he thought that the ſame colour was impreſſed upon glaſs by all the

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* Phil. Tranſ. Vol. liv. p. 208.

metals; and imagined that this circumſtance was a trace of the fundamental principles being the ſame in them all [*].

MR. DALIBARD obſerved, that, when a large pane of glaſs diſcharged itſelf, the poliſh was taken off at the place of the diſcharge, and that the track it left behind it was uſually, as he expreſſes it, in the *zig zag* form. With the piece of glaſs with which he made theſe diſcharges he pierced 160 leaves of paper. It contained 1200 ſquare inches [†].

MR. WILCKE ſays, that, if a ſmall piece of metal be hung in a ſmall ſilken ſtring, oppoſite to a part of a glaſs jar, made thin for the purpoſe; upon the ſpontaneous diſcharge through that place, the piece of metal will be driven off to the diſtance of five or ſix inches [‡].

MR. WINKLER fired the ſeeds of clubmoſs (lycopodium) by diſcharging a phial through a quantity of them. He alſo fired the *aurum fulminans* placed upon a piece of parchment, which was torn to pieces by the exploſion [§].

BY the electric ſhock, Signior Beccaria could melt borax and glaſs. But the moſt remarkable of his experiments with the electric ſhock are thoſe by which he *revivified metals*. This he did by making the exploſion between two pieces of the calces. In this manner he revivified ſeveral of the metals, and among others, zink. He even produced real quickſilver from cinnabar [‖]. In this caſe of revivification, he always obſerved ſtreaks of black beyond the coloured metallic ſtains, owing, as he imagined, to the phlogiſton driven thither from the parts that were vitrified, when the other part revivified the calx [¶].

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[*] Elettriciſmo artificiale, &c. p. 134, 135.

[†] Hiſtoire abrigée, p. 84.

[‡] Remarks on Franklin's Letters, p. 266.

[§] Phil. Tranſ. Vol. xxxviii. pt. ii. p. 773.

[‖] Lettere dell' elettriciſmo, p. 282.

[¶] Ibid. p. 255.

Another curious experiment he made with the electric ſhock, by diſcharging it through ſome braſs-duſt, ſprinkled between two plates of ſealing-wax. The whole was perfectly luminous and tranſparent[*]. An experiment which throws ſome light upon one of Mr. Haukſbee's.

With the electric ſhock he alſo made that capital experiment, on which he lays ſo much ſtreſs in his theory of thunder ſtorms, and by which he proves, that the electric matter forces into its paſſage all light conducting ſubſtances; by means of which it is enabled to paſs through a quantity of reſiſting medium, which it could not otherwiſe do. He put a narrow piece of leaf ſilver between two plates of wax, laying it acroſs the plates, but ſo as not quite to reach one of the ſides. The diſcharge being made through this ſtrip of metal, by bringing a wire oppoſite to the ſilver, at the place where it was diſcontinued; the ſilver was found melted, and part of it diſperſed all along the track that the electric matter took, between the plates of wax, from the ſilver to the wire[†]. An accident gave him occaſion to obſerve another fact of a ſimilar nature. He once, inadvertently, received the charge of a ſmall jar, through ſome ſmoke of ſpirit of nitre; when a hole was made in his thumb, where the fire entered; and which he thought could only have been made by the nitre, which was carried along with the electric fluid[‡].

Violently as the electric exploſion generally affects the human body, we have accounts of ſome perſons who could not be made to feel it; particularly three or four mentioned by Mr. Muſchenbroeck, among whom was a young woman[§]. I have alſo been told, that this was the caſe with a perſon near Leeds, who was at the ſame time a little paralytic.

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[*] Lettere dell' elettriciſmo, p. 257.
[†] Ibid. p. 248.
[‡] Ibid. p. 249.
[§] Monthly Review, October 1767, p. 250.

I SHALL cloſe the hiſtory of the Leyden phial for this period with the accounts of ſome extremely curious facts, which Mr. Canton gives me leave to publiſh relating to this ſubject. They certainly deſerve the utmoſt attention of philoſophers, and may probably throw ſome light upon the electricity of the tourmalin.

HE procured ſome thin glaſs balls, of about an inch and a half in diameter, with ſtems or tubes of eight or nine inches in length, and electrified them, ſome poſitively on the inſide, and others negatively, after the manner of charging the Leyden phial, and then ſealed them hermetically. Soon after, he applied the naked balls to his electrometer, and could not diſcover the leaſt ſign of their being electrical: but holding them to the fire, at the diſtance of five or ſix inches, they became ſtrongly electrical, in a very ſhort time, and more ſo when they were cooling. Theſe balls would, every time they were heated, give the electric fire to, or take it from other bodies, according to the *plus* or *minus* ſtate of it within them. Heating them frequently, he found, would ſenſibly diminiſh their power; but keeping one of them under water a week did not appear in the leaſt to impair it. That which he kept under water was charged the 22d of September 1760, was ſeveral times heated before it was kept in water, and had been heated frequently afterwards; and yet it ſtill retained its virtue to a conſiderable degree, on the 31ſt of October following, when he ſent an account of it to Dr. Franklin. The breaking two of his balls accidentally gave him an opportunity of meaſuring their thickneſs, which he found to be between ſeven and eight parts in 1000 of an inch.

THE balls mention in the account above, which was written ſix years ago, ſtill* retain their virtue, but in a leſs degree.

MR. LULLIN alſo found, that a glaſs tube charged, and hermetically ſealed, would ſhow ſigns of electricity when it was heated †.

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* In 1769.

† Diſſertatio Phyſica, p. 32.

SECTION VIII.

EXPERIMENTS AND OBSERVATIONS CONCERNING ELECTRIC LIGHT.

MY reader has been informed of the neceſſity I was under of dividing the buſineſs of this period of my hiſtory into ſeveral parts. He has already ſeen titles which he could not have expected from the diviſions of the preceding periods, but he would perhaps leaſt of all expect a diſtinct ſection upon electric light; and yet the experiments and obſervations which have been made, immediately relating to this ſubject, are ſo many, that they deſerve a place by themſelves. And I would rather err by making too many ſubdiviſions, than too few; becauſe, above all things, I would wiſh to preſerve perſpicuity, which is chiefly injured by crowding together things diſſimilar.

MANY experiments had been made very early, by Mr. Haukſbee and others, on electricity, and particularly electric light, in vacuo; but ſo little was, at that time, known of the nature of electricity in general, that, comparatively, little uſe could be made of thoſe experiments. Very fortunately, Dr. Watſon happened to turn his thoughts that way, after the great diſcovery of the accumulation of electricity in the Leyden phial; and by this means he diſcovered, that our atmoſphere, when dry, is the agent by which, with the aſſiſtance of other electrics per ſe, we were enabled to accumulate electricity upon non-electrics (he might have added electrics too), that is, to communicate to them a greater quantity of electricity than thoſe bodies naturally have. That upon the removal of the air, the electric fluid pervaded the vacuum to a conſiderable diſtance, and manifeſted its effects upon any non-electric ſubſtances by which it was terminated.

THIS he demonſtrated by one of the moſt beautiful experiments which the whole compaſs of electricity yet exhibits. He exhauſted a glaſs cylinder, three feet in length, and three inches in diameter, with a contrivance to let down a braſs plate, as far as he pleaſed, into it; in order to make it approach another plate, fixed near the bottom of the veſſel.

THIS cylinder, thus prepared, he inſulated, and obſerved, that when the upper plate was electrified, the electric matter would paſs from one plate to another, at the greateſt diſtance to which the braſs plates could be drawn; and that the braſs plate at the bottom of the cylinder was ſtrongly electrified, as if a wire had connected it with the prime conductor. It was a moſt delightful ſpectacle, he ſays, when the room was darkened, to ſee the electric matter in its paſſage through this vacuum; to obſerve, not as in the open air, ſmall bruſhes or pencils of rays, an inch or two in length, but coruſcations of the whole length of the tube, and of a bright ſilver hue. Theſe did not immediately diverge, as in the open air, but frequently, from a baſe apparently flat, divided themſelves into leſs and leſs ramifications, and reſembled very much the moſt lively coruſcations of the aurora borealis.

SOMETIMES he obſerved, that when the tube had been exhauſted in the moſt perfect manner, the electric fluid was ſeen to paſs between the braſs plates in one continued ſtream, of the ſame dimenſions throughout its whole length; which he thought demonſtrated, that the cauſe of that very powerful mutual repulſion of the particles of electric fire, which is ſeen in the open air, is more owing to the reſiſtance of the air, than to any natural tendency of the electricity itſelf. For, in the open air we obſerve that theſe bruſhes, when the electricity is ſtrong, diverge ſo much, as to form almoſt a ſpherical figure *.

He made this vacuum part of a circuit neceſſary to make the diſcharge of a phial; and, at the inſtant of the exploſion, there was ſeen a maſs of very bright embodied fire, jumping from one of the braſs plates in the tube to the other. But this did not take place when one of the plates was farther diſtant from the other than ten inches. If the diſtance was greater, the fire began to diverge, and loſe part of its force: and this force diminiſhed in proportion to its divergency, which was nearly as the diſtance of the two plates.

To find a more perfect vacuum for the paſſage of the electric fluid, he had recourſe to an excellent invention of Lord Charles Cavendiſh; who, by means of a long bent tube of glaſs, filled with mercury, and inverted, made all the bended part of it (which was above the mercury) the moſt perfect vacuum that can be made. This vacuum Dr. Watſon inſulated, and one of the baſons of the mercury being made to communicate with the conductor, when ſome non-electric ſubſtance touched the other, the electric matter pervaded the vacuum in a continued arch of lambent flame, and, as far as the eye could follow it, without the leaſt divergency.

Connecting one of the baſons with the machine, which was inſulated, the fire was ſeen pervading the vacuum in a contrary direction. And this he conſidered as the *experimentum crucis* of two principles which he had advanced before, viz. that electricity is furniſhed to the conductor, not by the excited electric, but from the non-electrics in contact with the rubber; and that we are able to take from, or add to that quantity of electricity, which is naturally inherent in bodies.

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* Phil. Tranſ. Vol. xlvii. p. 367.

HE alſo obſerved, that if, in the fore-mentioned circumſtances, the hand of a perſon ſtanding upon the floor was brought near the ſide of the glaſs, the coruſcations would dart themſelves that way in a great variety of forms, extremely curious to behold.

BUT the Doctor found, that even this vacuum did not conduct ſo perfectly as metals, or water; becauſe a perſon ſtanding upon the floor, and applying his finger to the upper braſs plate, received a ſmart ſtroke. This he conceived to ariſe from the electricity of the braſs being ſo much more rarefied than that of the body of the man who applied his finger *.

MR. WILSON engaged Mr. Smeaton, the inventor of a new and more perfect kind of air-pump, to make ſome electrical experiments in vacuo. The following is the account of them that he tranſmitted to Mr. Wilſon. They are, in ſeveral reſpects, ſimilar to thoſe made by Dr. Watſon, and yet are attended with a conſiderable variety of new circumſtances.

A GLASS veſſel, about one foot in length, and eight inches in its greateſt diameter, open at both ends, had one of its ends cloſed by a braſs ferule, which conſtituted one of the centers on which it turned; the other end was cloſed with a metal plate. In the center of this plate was a ſquare ſtem, which was applied to the arbor of a lath, by which the glaſs was turned round. On one ſide of this laſt plate was fixed a cork, by means of which the glaſs was ſcrewed upon the air-pump.

UPON rarefying the air within the glaſs about 500 times, and afterwards turning the glaſs in the lath, whilſt, at the ſame time, it was rubbed with his hand; a conſiderable quantity of lambent flame, variegated with all the colours of the rainbow, appeared within the glaſs, under the hand. This light was pretty

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* Phil. Tranſ. Vol. xlvii. p. 373.

ſteady in every reſpect, except that every part of it was perpetually changing colour.

WHEN a little air was let into the glaſs, the light appeared more vivid, and in a greater quantity, but was not ſo ſteady: for it would frequently break out into a kind of coruſcations, like lightning, and fly all about within the glaſs. When a little more air was let in, the flaſhing was continual, and ſtreams of blueiſh light ſeemed to iſſue from under his hand, within the glaſs, in a thouſand forms, with great rapidity; and appeared like a caſcade of fire. Sometimes it ſeemed to ſhoot out into the forms of trees, moſs, &c.

WHEN more air was let in, the quantity of light was diminiſhed, and the ſtreams compoſing the flaſhes narrower. The glaſs now required a greater velocity, and harder friction. Theſe circumſtances increaſed as more air was let in; ſo that, by ſuch time as the glaſs was one third full of air, theſe coruſcations quite vaniſhed, and a much ſmaller quantity of light appeared partly within, and partly without the glaſs. And when all the air was let in, the light appeared wholly without the glaſs, and much leſs in quantity than when the glaſs was in part exhauſted *.

MR. CANTON, in repeating Dr. Watſon's experiment with the Torricellian vacuum, obſerved one circumſtance attending it, which throws great light upon the Leyden phial. He obſerved, that when the excited tube was brought near one of the baſons of this machine (inſulated) a light was ſeen through more than half of the vacuum; which ſoon vaniſhed, if the tube was not brought nearer, but which appeared again as it moved farther off; and that this appearance might be repeated ſeveral times, without exciting the tube afreſh.

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* Wilſon's Eſſay, p. 216.

THIS experiment he conſidered as a kind of ocular demonſtration of the truth of Dr. Franklin's hypotheſis, that when the electric fluid was condenſed on one ſide of the glaſs, it was repelled from the other, if it met with no reſiſtance. Thus, at the approach of the excited tube, he ſuppoſed the fire to be repelled from the inſide of the glaſs ſurrounding the vacuum, and to be carried off through the columns of mercury, but to return again as the tube was withdrawn *.

THIS curious experiment Mr. Canton, as he informed me, ſhowed and explained to Mr. Wilſon; who afterwards expatiated upon it, in a book publiſhed by him and Dr. Hoadley in conjunction, intitled *Obſervations on a Series of Electrical Experiments*; in a note of which, p. 28, he ſays, "Mr. Canton has "taken notice of this vaniſhing and returning of the light."

MR. CANTON has ſince diverſified this beautiful experiment, by bringing the excited tube to another glaſs tube, exhauſted, and hermetically ſealed; by which means he exhibits the perfect appearance of an aurora borealis. The flame from one of its extremities, which is in a manner coated by the hand which holds it, will dart to the other extremity, at uncertain intervals of time, for near a quarter of an hour together, without repeating the application of the excited tube.

WHEN it was generally agreed among electricians, that what had been called vitreous and reſinous electricity were in reality a redundancy of the electric fluid in one caſe, and a deficiency in the other; and when, in conſequence of this ſuppoſition, the one was called poſitive, and the other negative electricity; there ſtill remained ſome doubt which of the two was poſitive, and which negative. Mr. Wilſon, in a paper read at the Royal Society, December the 6th, 1759, recites an experiment which, he thought, put the matter beyond all diſpute, and abſolutely determined, that what had been called vitreous was really poſitive, and what had been called reſinous was negative; as, indeed, had generally been ſuppoſed, though, as Mr. Wilſon thought, without ſufficient reaſon, notwithſtanding what had been advanced by Dr. Franklin, and Mr. Canton upon that ſubject.

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* Phil. Tranſ. Vol. xlviii. pt. i. p. 356. It has been ſeen p. 76, that this obſervation, of the return of the electric light *in vacuo*, was made before the diſcovery of the Leyden phial by Mr. Grummert of Biala in Poland; but this was unknown both to Dr. Watſon and Mr. Canton.

Repeating the beautiful experiment mentioned before, as firſt contrived by Lord Charles Cavendiſh, he ſays he attended to a circumſtance which ſeemed to have been overlooked by Dr. Watſon, who publiſhed the account of it. This was a ſingular appearance of light upon one of the ſurfaces of the quickſilver. To obſerve this remarkable appearance to more advantage, Mr. Wilſon let a ſmall quantity of air into the tube, by which means four columns of quickſilver were obtained, and conſequently ſix viſible ſurfaces, in one of the legs of the inverted tube. He then electrified the mercury in the other leg, while the mercury on the oppoſite ſide had a communication with the earth, when, the room being dark, the ſtream of electric light was viſible through the whole length of the vacuum, and its general appearance was of a ſeeming uniform denſity; except at the upper ſurfaces of each column, where about one tenth of an inch above the ſurface, the light was always conſiderably brighter; whereas the under ſurfaces exhibited no ſuch appearance, the light being rather leſs bright in thoſe places than in the general appearance of the whole illuminated vacuum.

This luminous appearance Mr. Wilſon aſcribed to the reſiſtance the fluid met with at the upper ſurface of the quickſilver, in endeavouring to get into it. He therefore inferred that excited glaſs electrified bodies poſitively, or gave them a greater quantity of the electric fluid than they had.

Electrifying, in the ſame place, with a cylinder of roſin, inſtead of glaſs, the luminous appearances were all on the under ſurfaces of the columns of quickſilver; from which he inferred, that roſin electrified bodies negatively, depriving them of part of the electric fluid which they naturally had; or, as he expreſſes it, occaſioning a current of electric fluid to ſet the contrary way.

These luminous appearances, Mr. Wilſon alſo conſidered, as a ſtrong confirmation of the exiſtence of a *medium*, at or near the ſurfaces of bodies, which hindered the entrance or exit of the electric fluid. A doctrine which Mr. Wilſon had advanced, and laid great ſtreſs upon on ſeveral other occaſions *.

The arguments which to Mr. Wilſon appeared concluſive, in proof of what is commonly ſuppoſed, that glaſs electrifies *plus*, and ſulphur, &c. *minus*, did not appear ſo to Mr. Æpinus; though he acknowledges that the knobs of light in the vacuum did, in common with many other appearances, prove a real difference between the two electricities; and thought that it was very eaſy to conceive, that when an elaſtic fluid iſſues from a body, it ſhould be denſer near the ſurface from whence it iſſues, than where it finds more liberty to expand itſelf. He might have added, that this might have been expected, from the mutual attraction which is ſuppoſed to ſubſiſt between the electric fluid, and other bodies. But Æpinus did not expreſsly mention this circumſtance. Mr. Wilſon, therefore, makes light of the objection; and adds, that when he related the experiment with the bent tube, in his letter to Dr. Heberden, he omitted ſome phenomena attending the fact, which greatly favoured the doctrine he advanced. If, ſays he, when glaſs is electrified, and applied to the firſt column, we ſuffer the electric fluid to paſs along the tube in ſmall quantities only, and at ſhort intervals, little luminous ſtreams will be ſeen to move from the firſt to the ſecond column of quickſilver, and conſequently from the glaſs. The like appearances happen, but in a contrary direction, when roſin or amber is made uſe of, and applied to the ſame column. Glaſs, therefore, he concludes, electrifies *plus*, or fills bodies with more of this fluid than belongs to them naturally, and roſin, &c. *minus* *.

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* Phil. Tranſ. Vol. li. pt. i. p. 308.

This controverſy took its riſe, in ſome meaſure, from a deception: for Mr. Canton informs me, and gives me leave to inform the public, that the light which Mr. Wilſon takes notice of, as appearing on one ſurface of the mercury in the double barometer of Lord Charles Cavendiſh, and which Mr. Wilſon takes to be a proof of the exiſtence of a medium on the ſurface of bodies, which hinders the entrance or exit of the electric fluid to ſome degree, he found to be cauſed by nothing but common air. For if the Torricellian vacuum be properly made, no difference of light can be ſeen on the ſurfaces of the columns of mercury; but if as much air be let into the vacuum, as will make each column of mercury a quarter of an inch ſhorter than that of a good barometer, the light will appear as Mr. Wilſon has deſcribed it. When Mr. Wilſon ſuppoſed that Dr. Watſon, when he made the experiment of the Torricellian vacuum, did not attend to the ſingular appearance of light on one of the ſurfaces of the mercury, he little ſuſpected that if the vacuum Dr. Watſon made was free from air, there was no ſuch ſingular appearance of light to be attended to. Air, Mr. Canton adds, muſt be condenſed near the ſurface of all bodies that attract it; and will, therefore, be ſome hindrance to the exit, or entrance of the electric fluid, except the bodies be very ſharply pointed.

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* Phil. Tranſ. Vol. liii. p. 438. p. 441.

Some curious obſervations relating to electric light were made by Mr. Wilcke. Rubbing two pieces of glaſs together in the dark, he obſerved a vivid phoſphoreal light: which, however, threw out no rays, but adhered to the place where it was excited. It was attended with a ſtrong phoſphoreal ſmell, but with no attraction or repulſion. From this experiment he inferred, that friction alone would not excite electricity, ſo as to be accumulated upon any body; and that to produce this effect, the bodies rubbed together muſt be of different natures, with reſpect to their attracting the electric fluid. He, moreover, imagined, that all examples of phoſphoreal light, without attraction, were owing to the ſame excitation of electricity, without the accumulation of it. Such he imagined to be the caſe of light emitted by the Bolognian ſtone, cadmea fornacum, rotten wood, pounded ſugar, and glaſs of all kinds *.

A tube excited with a woollen cloth, on which white wax or oil had been put, he ſays, threw out flames; each of which when examined, appeared to riſe out of a little protuberance of fire. The flame was one, and very narrow at the bottom, but farther from the tube it divided into ſeveral ramifications; which always leaned to thoſe parts of the tube which were the leaſt excited, or to conductors in the neighbourhood †.

He ſays that, upon preſenting a finger or other non-electric to an excited negative electric, a cone of light is formed; the baſe of which is at the finger, or other non-electric, and the apex at the electric, on the ſurface of which it ſpreads to a conſiderable diſtance all round ‡.

Sometimes, he had ſeen fiery particles thrown laterally from an irregular electric ſpark, which ſhone like ſtars, and were very like thoſe which are produced by the colliſion of flint and ſteel §.

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* Wilcke, p. 123, 124.

† Ibid. p. 125.

‡ Ibid. p. 127.

§ Ibid. p. 130.

SUSPENDING various balls from his conductor, and preſenting others to them, which were ſometimes of glaſs and ſometimes of metal, and varying them in every manner poſſible, he always found (except when two metal balls were uſed) that the light between them formed a cone, the baſe of which was always on the body which was poſitive, and the apex on that which was negative. He ſays that this criterion is ſufficient to diſtinguiſh the two electricities from one another.

HE obſerves that, at the apex of a cone iſſuing from pointed bodies, electrified poſitively, there is a cylindrical ſpark, out of which lucid rays, like a river, are darted. Theſe rays, he ſays, form a lucid cone, the apex of which is turned towards the point from which the fire proceeds. Sometimes from the apex, or at ſome diſtance from it, there is a lucid point, which, he ſays, Hauſenius calls *the fire of the ſecond kind*, out of which flew ſtreams of fire. The ſtreams never iſſue from the electrified body itſelf, but always from this lucid point. He ſays, moreover, that this lucid point at the extremity of an electrified body, and which throws out lucid rays, forms the diſtinctive character of the poſitive cone *.

A NEGATIVE cone, he ſays, is ſmall, conſiſting of very ſlender filaments, which immediately adhere to the point at which the light enters, or to its ſides; and, if accurately examined, ſeems to form little cones, the baſes of which reſt upon the body.

WHEN he afterwards comes to conſider the cauſe of negative cones of light, he owns himſelf to be at a great loſs how to do it.

MR. WILCKE put Engliſh phoſphorus upon a pointed body, which, in the dark, rendered the whole viſible; and when he ſuſpended this pointed body perpendicularly, the phoſphoreal vapours were ſeen to aſcend; but upon electrifying it, as it hung in the ſame direction, the vapours were carried downwards, and formed a very long cone, extending out of the middle of the cone of electric light, which was ſeen perfectly diſtinct from it. When the electrification was diſcontinued, the phoſphoreal vapour aſcended as at firſt. From this depreſſion of the phoſphoreal effluvia Mr. Wilcke infers the efflux of the electric fluid from the point, and upon the ſurface, and not only through the ſubſtance of the pointed body. It is pity that he did not try this curious experiment with pointed bodies electrified negatively. He would certainly have found the ſame depreſſion of the phoſphoreal effluvia, and would, probably, have retracted his concluſion concerning this proof of the efflux *.

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* Wilcke, p. 132.

MR. WILCKE alſo thought it to be a proof, that the electric matter did not only flow out of the ſubſtance of electrified bodies, but upon the ſurface of them, that a metallic ring, projecting ever ſo little beyond the point of a wire on which it had been put, prevents the appearance of the lucid point.

THE laſt obſervation which I ſhall recite of Mr. Wilcke concerning electric light is, that if a point not electrified be oppoſed to a point electrified poſitively, the cones of light, which, in other circumſtances, would appear upon both of them, diſappear; but that if a poſitive cone be oppoſed to a negative cone, they both preſerve their own characteriſtic properties †.

SIGNIOR BECCARIA was of opinion, that the direction of the electric fluid may be determined from the phenomena of pointed bodies. The *pencil* (by which he means the electric fire at a point electrified poſitively) he ſays, contracts as it approaches a flat piece of metal not electrified; whereas the *ſtar* (by which he means the electric fire at a point electrified negatively) expands in the ſame circumſtances, and has a ſmall cavity near the point towards the large ſuperficies. The pencil is attended with a ſnapping noiſe, the ſtar makes little or no noiſe. He hardly gives any reaſon for the firſt of theſe phenomena; he only ſays, that ſuch is the neceſſary conſequence of a fluid iſſuing out of, or entering into a point. But the greater noiſe made by the pencil, he thought was made by the impulſe given by the electric matter to the air, cauſing it to vibrate: and this muſt be greater when the fluid is thrown from the point into the air, than when it comes through different portions of the air, and meets in one point *.

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* Wilcke, p. 134.

† Ibid. p. 140.

When two points are oppoſed to one another, he ſays, the phenomena are much the ſame in both †.

Signior Beccaria obſerved that hollow glaſs veſſels, of a certain thinneſs, exhauſted of air, gave a light when they were broken in the dark. By a beautiful train of experiments, he found, at length, that the luminous appearance was not occaſioned by the breaking of the glaſs, but by the daſhing of the external air againſt the inſide, when it was broke. He covered one of theſe exhauſted veſſels with a receiver, and letting the air ſuddenly on the outſide of it, obſerved the very ſame light. This he calls his *new invented phoſphorus* ‡.

This excellent philoſopher produced a moſt beautiful appearance of electric light in the following manner. He conveyed poſitive electricity to a braſs ball, ſuſpended by a wire, within an exhauſted receiver, when, upon preſenting to it another ball *in vacuo*, the lower hemiſphere of the former was moſt beautifully illuminated, with a viſible electric atmoſphere. When he conveyed negative electricity to the ball, the ſame beautiful illumination was obſerved on the ball preſented to it. This experiment, he ſays, is a very delicate one, requiring great patience and dexterity, in adjuſting the diſtances, &c. in order to make it ſucceed perfectly *.

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* Elettriciſmo artificiale, p. 63. † Ibid.

‡ Lettere dell' ellettriciſmo, p. 365, &c.

THAT electric light is more ſubtle and penetrating, if one may ſay ſo, than light produced in any other way is manifeſt from ſeveral experiments, particularly the remarkable one of Mr. Haukſbee; but none prove it ſo clearly as ſome made by the ingenious Mr. Lane, who gives me leave to mention theſe.

WHEN he had, for ſome different purpoſe, made the electric ſhock paſs over the ſurface of a piece of marble, in the dark; he obſerved, that the part over which the fire had paſſed was luminous, and retained that appearance for ſome time. No ſuch effect of the electric ſhock having ever been obſerved before, he repeated the experiment with a great variety of circumſtances, and found it always anſwered with all calcarious ſubſtances, whether animal or mineral, and eſpecially if they had been burnt into lime. And, as far as he had tried, many more ſubſtances would retain this light than would not do it; among others ſeveral vegetable ſubſtances would do it, particularly white paper. Tiles and bricks were luminous, but not tobacco-pipe clay, though well burnt.

THAT gypſeous ſubſtances, when calcined, were luminous, appeared from bits of images made of plaiſter of Paris; and of this claſs, he ſays, is the famous Bolognian ſtone. But many bodies, he found, were luminous after the electric ſtroke, which were not apparently ſo, when expoſed to the rays of the ſun.

HE made theſe curious experiments by placing the chains, or wires that led from the conductor to the outward coating of his jar, within one, two, or three inches (according to the ſtrength of the charge) from one another, on the ſurface of the body to be tried, and diſcharging a ſhock through them. If the ſtone was

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* Phil. Tranſ. Vol. lvi. p. 107.

thin, he found, that if one chain was placed at the top, and the other at the bottom, it would appear luminous on both ſides after the exploſion.

MR. CANTON, to whom theſe experiments were communicated, clearly proved, that it was the *light* only that the ſubſtances retained, and nothing peculiar to electricity; and, moreover, after frequent trials, diſcovered a compoſition, which retains both common light, and that of electricity, much more ſtrongly than either the Bolognian ſtone, or any other known ſubſtance whatever. With this new phoſphorus he makes a great number of moſt beautiful experiments. The flaſh made by the diſcharge of a common jar, within an inch of a circular piece of it, of about two inches and a half in diameter, will illuminate it ſo much, that the figures on a watch plate may be eaſily diſtinguiſhed by it in a darkened room, and it will retain the light half an hour *.

I SHALL cloſe this ſection of experiments and obſervations on electric light, with an account of a remarkable appearance which occurred to Mr. Hartman. When he had been making experiments four or five hours together, in a ſmall room, and after going out of it, returned ſoon, with a lighted candle in his hand, walking pretty ſwiftly; he perceived a ſmall flame to follow him, at the diſtance of about a ſtep and a half, but it vaniſhed when he ſtopped to examine it. He was a good deal alarmed at the appearance at firſt, but afterwards imagined it to be occaſioned by the aſcenſion of the ſulphur, which had been thrown into the air by the violent continued electrification †.

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* See a particular account of this compoſition, and experiments with it, in my *Hiſtory of Diſcoveries relating to Viſion, Light, and Colours*, p. 371.

† Abhandlung, p. 135.

SECTION IX.

THE ELECTRICITY OF THE TOURMALIN.

THIS period of my hiſtory furniſhes an entirely new ſubject of electrical inquiries; which, if properly purſued, may throw great light upon the moſt general properties of electricity. This is the *Tourmalin:* though, it muſt be acknowledged, the experiments which have hitherto been made upon this foſſil ſtand like exceptions to all that was before known of the ſubject.

THE tourmalin, as Dr. Watſon ſuppoſes, was known to the ancients under the name of the *lyncurium.* All that Theophraſtus ſays concerning the lyncurium agrees with the tourmalin, and with no other foſſil that we are acquainted with. He ſays, that it was uſed for ſeals, that it was very hard, that it was endued with an attracting power like amber, and that it was ſaid, particularly by Diocles, to attract not only ſtraws, and ſmall pieces of wood, but alſo copper and iron, if beaten very thin; that it was pellucid, of a deep red colour, and required no ſmall labour to poliſh it. The account which paſſed current among the ancients concerning the origin of this ſtone was fabulous, which made Pliny think that all that was ſaid of it was fabulous too.

THIS ſtone, though not much attended to by European philoſophers, till very lately, is common in ſeveral parts of the Eaſt Indies, and more particularly in the iſland of Ceylon, where it is called by the natives *tournamal.* In this iſland the Dutch became acquainted with it, and by them it is called *aſchentrikker*, from its property of attracting aſhes, when it is thrown into the fire.

The firſt account we have had, of late years, concerning this extraordinary ſtone is in the Hiſtory of the Royal Academy of Sciences at Paris for the year 1717; where we are told, that Mr. Lemery exhibited a ſtone, which was not common, and came from Ceylon. This ſtone, he ſaid, attracted and repelled ſmall light bodies, ſuch as aſhes, filings of iron, bits of paper, &c.

Linnæus, in his Flora Zeylonica, mentions this ſtone under the name of *lapis electricus*, and takes notice of Mr. Lemery's experiments.

Notwithstanding this, no farther mention was made of this ſtone and its effects till ſome years after; when the Duc De Noya, in his letter to Mr. Buffon, preſented to the Royal Society, informed us, that when he was at Naples, in the year 1743, the Count Pichetti, ſecretary to the king, aſſured him, that, during his ſtay at Conſtantinople, he had ſeen a ſmall ſtone called *tourmalin*, which attracted and repelled aſhes. This account the Duc De Noya had quite forgotten, but being in Holland in the year 1758, he ſaw, and purchaſed two of thoſe ſtones. With theſe, in company with Meſſrs. Daubenton and Adamſon, he made a great number of experiments, of which he favoured the public with a particular account *.

But prior to the Duc De Noya's experiments, Mr. Lechman had acquainted Mr. Æpinus with the attractive power of the tourmalin, and furniſhed him with two of them, on which he made many experiments; the reſult of which he publiſhed in the Hiſtory of the Academy of Sciences and Belles Lettres at Berlin for the year 1756. The ſubſtance of the memoir is as follows.

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* Phil. Tranſ. Vol. li. pt. i. p. 395.

THE tourmalin has always, at the ſame time, a poſitive and a negative electricity; one of its ſides being in one ſtate, and the other in the oppoſite; and this does not depend on the external form of the ſtone. Theſe electricities he could excite in the ſtrongeſt degree, by plunging the ſtone in boiling-water.

IF one ſide of the tourmalin be heated more than the other (as if it be laid upon a hot cake of metal) each of the ſides acquire an electricity oppoſite to that which is natural to it; but if left to itſelf, it will return to its natural ſtate.

IF one of the ſides of the tourmalin be rubbed, while the other is in contact with ſome conductor communicating with the ground; the rubbed ſide is always poſitive, and the other negative. If neither ſide be in contact with a conductor, both become poſitive. If, in the former of theſe caſes, the tourmalin be rubbed, ſo as to acquire a ſenſible heat, and the ſide which is naturally poſitive be made negative, it will upon ſtanding to cool, return to its natural ſtate; but if it have acquired no ſenſible heat, it will not return to its natural ſtate while any kind of electricity remains. If it be heated, even when it is rubbed and inſulated, (in which caſe both ſides become poſitive) it will ſtill return to its natural ſtate upon cooling.

THE Duc De Noya mentions theſe experiments of Mr. Æpinus, but does not admit of a *plus* and *minus* electricity belonging to the tourmalin when heated. On the contrary, he ſays, that both the ſides are electrified *plus*, but one of them more than the other, and that it was the difference between thoſe degrees which led Mr. Æpinus into his miſtake*.

THE tourmalin was introduced to the notice of the Engliſh philoſophers by Dr. Heberden, who fortunately recollecting to have ſeen one of them, many years before, in the poſſeſſion of Dr. Sharp at Cambridge (and it was the only one known in England at that time) procured it for Mr. Wilſon; who, though it was but a ſmall one, repeated with it moſt of the experiments of Mr. Æpinus, ſo far as to ſatisfy himſelf that his opinion of its poſitive and negative power was well founded.

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* Phil. Tranſ. Vol. lvii. pt. i. p. 315.

AFTERWARDS, Dr. Heberden, ever deſirous of extending the bounds of ſcience, procured ſome of theſe ſtones from Holland, and put them into the hands of thoſe perſons who were likely to make the beſt uſe of them, particularly Mr. Wilſon and Mr. Canton; in whoſe hands they were not lodged in vain, as will appear by the brief account I ſhall ſubjoin of their experiments upon them.

MR. WILSON's obſervations are too many, and too particular to be all inſerted in this work. The reſult of them was, in the main, the ſame with that of Mr. Æpinus, eſtabliſhing the opinion of the two different powers of this ſtone; but, contrary to Mr. Æpinus, he aſſerts, that when the ſides of the tourmalin are unequally heated, it exhibits that ſpecies of electricity which is natural to the hotter ſide, that is, the tourmalin is *plus* on both ſides, when the *plus* ſide is the hotter; and *minus* on both ſides, when the *minus* ſide is the hotter.

UPON this Mr. Æpinus repeated all his former experiments, and ſtill found the reſult of them agreeable to his former concluſion, and contrary to that of Mr. Wilſon. Mr. Wilſon alſo repeated his, without any variation in the event, and imagined the difference between him and Mr. Æpinus might ariſe from the different ſizes of the tourmalins they made uſe of, or from their different manner of making the experiments. And it is evident, from the deſcription of both their apparatuſes, that of Mr. Wilſon was much better calculated for the purpoſe of accurate experiments than that of Mr. Æpinus. Mr. Wilſon alſo uſed a greater variety of methods of communicating heat to his tourmalins. He both plunged them in boiling water, held them to the flame of a candle, and expoſed them to heated inſulated electrics *.

THOUGH the detail of all Mr. Wilſon's experiments would be, as I obſerved before, much too long for my purpoſe, I cannot help relating one of them, which was made with the laſt mentioned method of treating. He heated one end of a glaſs tube red-hot, and when he had expoſed what both he and Mr. Æpinus call the negative ſide of the tourmalin to it; he obſerved that about three inches of the heated part of the glaſs were electrified *minus*, though the glaſs beyond that was electrified *plus*, and continued ſo even after the glaſs was cold, the electric fluid having paſſed from the tourmalin to the glaſs; ſince theſe were the ſame appearances that were produced by preſenting an excited tube to the heated glaſs.

HE then applied the *plus* ſide of the tourmalin to the ſame heated glaſs, and found that the tube was electrified *minus*, above a foot in length, without the leaſt appearance of a *plus* electricity beyond the *minus* one, as in the other experiment; and this *minus* electricity appeared when the tube was nearly cold. In this caſe he judged that the electric fluid had paſſed from the glaſs to the tourmalin.

MR. WILSON imagined that the tourmalin, as well as glaſs, was permeable to the electric fluid, and that the reſiſtance to its entering the ſubſtance of it was leſs on what he calls the negative than on the poſitive ſide. Theſe concluſions he drew from the two following experiments. Rubbing the poſitive ſide of the ſtone ſlightly, he found both ſides electrified *plus*, but rubbing the negative ſide in the ſame manner, both ſides were electrified *plus* more ſtrongly than before *.

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* Phil. Tranſ. Vol. liii. p. 436, &c.

Several experiments led Mr. Wilſon to conclude that the tourmalin reſiſted the exit and entrance of the electric fluid conſiderably leſs than glaſs, or even than amber; and upon the whole he infers, that the tourmalin differs in nothing from other electric bodies but in acquiring electricity by heat †.

Examining a great number of tourmalins, he found that a line drawn from the *plus* part, through the center of the ſtone, would always paſs through the *minus* part.

Almost all theſe tourmalins he greaſed over, and whilſt they were warm enough to preſerve the greaſe liquid, he tried each tourmalin ſeparately, but found no alteration in the virtue of the ſtone, except that it was a little weakened; though it is well known, he ſays, that moiſture of any ſort readily conducts the electric fluid. If, therefore, the tourmalin had not a fixed kind of electricity, the *plus* and *minus* electricity, obſervable on the two ſides of the ſtone, muſt, by this treatment, have united and deſtroyed each other. From this circumſtance Mr. Wilſon concluded, that the tourmalin ſuffered the electric fluid to paſs through it only in one direction, and that in this it bore ſome analogy to the loadſtone; having, as it were, two electric poles, which are not eaſily deſtroyed or altered ‡. But he was not aware how imperfect a conductor oil or greaſe of any kind is.

This induced him to try whether, like the loadſtone, the tourmalin would loſe its virtue after being made red-hot; but, though he kept two of them in a ſtrong fire for half an hour, he could not perceive the leaſt alteration in them: but plunging one in water, while it was red-hot entirely deſtroyed its virtue, and

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* Phil. Tranſ. Vol. li. pt. i. p. 327. † Ibid. p. 329. ‡ Ibid. p. 337, 338.

gave it the appearance of having been ſhivered in many parts without breaking *. He obſerves that when a tourmalin, which he had from Dr. Morton, was held between the eye and the light, and viewed in the direction through which the electric fluid is found to paſs, it appears of a darker colour conſiderably than when it is viewed at right angles to the former direction. This appearance, he ſays, obtains in many other tourmalins, eſpecially when they happen to be as conveniently ſhaped †.

Notwithstanding the attention given to this ſubject by Mr. Æpinus and Mr. Wilſon, the moſt important diſcovery relating to the electricity of the tourmalin was reſerved for Mr. Canton; who, in a paper read at the Royal Society in the ſame month with that above mentioned of Mr. Wilſon, viz. December 1759, obſerves, that the tourmalin emits and abſorbs the electric fluid only by the increaſe or diminution of its heat. For if the tourmalin, he ſays, be placed on a plain piece of heated glaſs or metal, ſo that each ſide of it, by being perpendicular to the ſurface of the heated body, may be equally heated; it will, while heating, have the electricity of one of its ſides poſitive, and that of the other negative. This will, likewiſe, be the caſe when it is taken out of boiling water and ſuffered to cool; but the ſide which was poſitive while it was heating will be negative while it is cooling, and the ſide which was negative will be poſitive.

In this paper Mr. Canton refers to the Gentleman's Magazine for the month of September before, in which he had publiſhed the reſult of ſome experiments he had made on a tourmalin which he had procured from Holland. The propoſitions he there lays down are ſo few, and compriſe the principal part of what is known upon this ſubject in ſo conciſe and elegant a manner, that I ſhall recite them all in this place.

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* Phil. Tranſ. Vol. li. pt. i. p. 338.

† Ibid. Vol. liii. p. 448.

1. When the tourmalin is not electrical or attractive, heating it, without friction, will make it ſo; and the electricity of one ſide of it (diſtinguiſhed by A) will be poſitive, and that of the other ſide (B) will be negative.

2. The tourmalin not being electrical will become ſo by cooling; but with this difference, that the ſide A will be negative, and the ſide B poſitive.

3. If the tourmalin, in a non-electrical ſtate, be heated, and ſuffered to cool again, without either of its ſides being touched; A will be poſitive, and B negative, the whole time of the increaſe and decreaſe of its heat.

4. Either ſide of the tourmalin will be poſitive by friction, and both may be made ſo at the ſame time.

These, ſays he, are the principal laws of the electricity of this wonderful ſtone; and he adds, if air be ſuppoſed to be endued with ſimilar properties, i. e. of becoming electrical by the increaſe or diminution of its heat (as is probable, if its ſtate before and after a thunder ſtorm be attended to) thunder clouds both poſitive and negative, as well as thunder guſts, may eaſily be account for.

These capital diſcoveries were made before Mr. Canton had received of Dr. Heberden the tourmalins above mentioned. When thoſe came to his hands he was enabled to make ſeveral new and curious experiments, which I have leave to publiſh.

He put one of the tourmalins, which was of the common colour into the flame from a blow-pipe and burnt it white; when he found that its electrical property was intirely deſtroyed. The electricity of another was only in part deſtroyed by the fire. Two other tourmalins he joined together, when they were made ſoft by fire, without deſtroying their electrical property. The virtue of another was improved by its being melted at one end; and he found (contrary to what Mr. Wilſon had obſerved of an-

another tourmalin, which he treated in the ſame manner) that one tourmalin retained its electrical property after it had been frequently made red-hot, and, in that ſtate, put into cold water.

BUT the moſt curious of his experiments were made upon a large irregular tourmalin, about half an inch in length, which he cut into three pieces; taking one part from the poſitive, and another from the negative end. Trying theſe pieces ſeparately, he found the outer ſide of the piece which he cut from the end that was negative when cooling was likewiſe negative when cooling, and that the outer ſide of that piece which was cut from the end that was poſitive when cooling was likewiſe poſitive when cooling; the oppoſite ſides of both pieces being, agreeable to the general law of the electricity of the tourmalin, in a contrary ſtate.

THE middle part of the ſame tourmalin was affected juſt as it had been when it was intire; the poſitive end remained poſitive, and the negative end continued negative. The ſame he had alſo obſerved of two other tourmalins, each about the ſize of this, which were alſo cut out of a large one.

ON January the 8th, 1762, Mr. Canton took the Doctor's large tourmalin (which Mr. Wilſon has given a deſcription of in the fifty-firſt volume of the Philoſophical Tranſactions, p. 316.), and having placed a ſmall tin cup of boiling-water on one end of his electrometer, which was ſupported by warm glaſs, while the pith balls were ſuſpended at the other end; he dropped it into the water, and obſerved, that during the whole time of its being heated, and alſo while it was cooling in the water, the balls were not at all electrified.

TILL the year 1760 it had been ſuppoſed, that, of all electric ſubſtances, the tourmalin alone poſſeſſed the property of being excited by heating and cooling; but in the beginning of that year, Mr. Canton having had an opportunity of examining a variety of gems, by the favour of Mr. Nicolas Criſp a jeweller in Bow Church-yard, firſt found the *Brazil topaz* to have the electrical properties of the tourmalin. The largeſt he met with he put into the hands of Dr. Heberden, who returned it November the 27th, 1760, and ſent with it the tourmalin above mentioned.

IN September 1761, Mr. Wilſon (who had been informed of Mr. Canton's diſcovery) met with ſeveral other gems, of different ſizes and colours, that reſembled the tourmalin with reſpect to electrical experiments. The moſt beautiful of them were ſomething like the ruby, others were more pale, and one inclining to an orange colour. In point of hardneſs and luſtre, they were nearly the ſame with the topaz.

FROM all his experiments upon theſe gems he thought it was abundantly evident, that the direction of the fluid did not depend upon the external figure of the gem, but upon ſome particular internal make and conſtitution of it. And that there is ſome ſuch natural diſpoſition in all gems affording theſe appearances may be collected, he ſays, from another curious ſpecimen of the tourmalin kind, which is green, and formed of long ſlender cryſtals with ſeverals ſides; many of which are found ſticking together, and are brought from South America.

THESE gems, numbers of which were furniſhed him by Mr. Emanuel Mendes Da Coſta, he not only found to be like the tourmalin with reſpect to electric appearances; but that the direction of the electric fluid moving therein was always along the grain or ſhootings of the cryſtals, one end of it being electrified *plus*, and the other end *minus*. And that the fluid is more diſpoſed to paſs in that direction than in another, he thought, might be farther collected from what has been obſerved on the grain of the loadſtone by Dr. Knight; who found that though the magnetic poles of the natural loadſtone might be varied in any direction, yet that the ſame load-ſtone admitted of being made much more magnetical along the grain than acroſs it.

FROM theſe experiments and obſervations Mr. Wilſon inferred by analogy, that the electric fluid, flowing through all theſe ſtones and gems, moves in that direction in which the grain happens to lie; and that the reaſon of this is, that the reſiſtance which the fluid meets with in paſſing through the gem is leſs in that direction than in any other *.

IN a ſubſequent paper of Mr. Wilſon's, read at the Royal Society December the 23d, 1763, and March 1764, he recites ſeveral curious experiments on the effects of removing the tourmalin from one room to another, in which there was ſome difference of heat; the reſult of which exactly confirms Mr. Canton's diſcovery, that the ſide which is poſitive when heating is negative when cooling, and *vice verſa*. Upon a very nice examination, and during ſome favourable circumſtances, Mr. Wilſon ſays he has obſerved the tourmalin to be feebly electrified, when the thermometer varied up and down only one degree †.

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* Phil. Tranſ. Vol. lii. pt. ii. p. 443.

† Ibid. p. 457.

SECTION X.

DISCOVERIES THAT HAVE BEEN MADE SINCE THOSE OF DR. FRANKLIN, WITH RESPECT TO THE SAMENESS OF LIGHTNING AND ELECTRICITY.

THE year 1752 makes an æra in electricity no leſs famous than the year 1746, in which the Leyden phial was diſcovered. In the year 1752, was verified the hypotheſis of Dr. Franklin, of the identity of the matter of lightning and of the electric fluid; and his great project of trying the experiment by real lightning actually brought down from the clouds, was carried into execution.

THE French philoſophers were the firſt to diſtinguiſh themſelves upon this memorable occaſion, and the moſt active perſons in the ſcene were Mr. Dalibard and Delor, both zealous partiſans (as Mr. Nollet calls them) of Dr. Franklin. The former prepared his apparatus at Marly La Ville, ſituated five or ſix leagues from Paris, the other at his own houſe, on ſome of the higheſt ground in that capital. Mr. Dalibard's machine conſiſted of an iron rod forty feet long, the lower extremity of which was brought into a centry box, where the rain could not come; while, on the outſide, it was faſtened to three wooden poſts by long ſilken ſtrings defended from the rain. This machine happened to be the firſt that was favoured with a viſit from this ethereal fire. The philoſopher himſelf happened not to be at home at that time; but, in his abſence, he entruſted the care of his apparatus to one Coiffier a joiner, a man who had ſerved fourteen years among the dragoons, and on whoſe underſtanding and courage he could depend. This artiſan had all proper inſtructions given him, both how to make obſervations, and alſo to guard himſelf from any harm there might be from them; beſides being expreſsly ordered to get ſome of his neighbours to be preſent, and particularly to ſend for the curate of Marly, whenever there ſhould be any appearance of the approach of a thunder ſtorm. At length the long expected event arrived.

ON Wedneſday the 10th of May 1752, between two and three o'clock in the afternoon, Coiffier heard a pretty loud clap of thunder. Immediately he flies to the machine, takes a phial furniſhed with a braſs wire, and preſenting one end of the wire to the rod, he ſees a ſmall bright ſpark iſſue from it, and hears the ſnapping that it made. Taking a ſecond ſpark ſtronger than the former, and attended with a louder report, he calls his neighbours, and ſends for the curate. The curate runs with all his might, and the pariſhioners, ſeeing the precipitation of their ſpiritual guide, imagine that poor Coiffier had been killed with lightning. The alarm ſpreads through the village, and the hail which came on did not prevent the flock from following their ſhepherd. The honeſt eccleſiaſtic arriving at the machine, and ſeeing there was no danger, took the wire into his own hand, and immediately drew ſeveral ſtrong ſparks, which were moſt evidently of an electrical nature, and completed the diſcovery for which the machine was erected.

THE thunder cloud was not more than a quarter of an hour in paſſing over the zenith of the machine, and there was no thunder heard beſides that ſingle clap. As ſoon as the ſtorm was over, and no more ſparks could be drawn from the bar, the curate wrote a letter to Mr. Dalibard, containing an account of this remarkable experiment, and ſent it immediately by the hands of Coiffier himſelf.

HE ſays, that he drew ſparks from the bar of a blue colour, an inch and a half in length, and which ſmelled ſtrong of ſulphur. He repeated the experiment at leaſt ſix times in the ſpace of about four minutes, in the preſence of many perſons, each experiment taking up the time, as he, in the ſtile of a prieſt expreſſes himſelf, of a *pater* and an *ave*. In the courſe of theſe experiments he received a ſtroke on his arm, a little above the elbow, but he could not tell whether it came from the braſs wire inſerted into the phial, or from the iron bar. He did not attend to the ſtroke at the time he received it, but, the pain continuing, he uncovered his arm when he went home, in the preſence of Coiffier; and a mark was perceived round his arm, ſuch as might have been made by a blow with the wire on his naked ſkin; and afterwards ſeveral perſons, who knew nothing of what had happened, ſaid that they perceived a ſmell of ſulphur when he came near them.

COIFFIER told Mr. Dalibard, that for about a quarter of an hour before the curate arrived, he had, in the preſence of five or ſix perſons, taken much ſtronger ſparks than thoſe which the curate mentioned *.

EIGHT days after, Mr. Delor ſaw the ſame thing at his own houſe, although only a cloud paſſed over, without either thunder or lightning †.

THE ſame experiments were afterwards repeated by Mr. Delor, at the requeſt of the king of France, who, it is ſaid, ſaw them with the greateſt ſatisfaction, and expreſſed a juſt ſenſe of the merit of Dr. Franklin. Theſe applauſes of the king excited in Mr. De Buffon, Dalibard, and Delor, a deſire of verifying Dr.

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* Dalibard's Franklin, Vol. ii. p. 109, &c.

† Nollet's Letters, Vol. i. p. 9.

Franklin's hypotheſis more completely, and of purſuing his ſpeculations upon the ſubject.

Mr. Delor's apparatus in Paris conſiſted of a bar of iron ninety-nine feet high, and anſwered rather better than that of Mr. Dalibard, which, as was obſerved before, was only forty feet high. But as the quantity of electricity which they could procure from the clouds, in theſe firſt experiments, was but ſmall, they added to this apparatus what they called a *magazine* of electricity, conſiſting of many bars of iron inſulated, and communicating with the pointed iron rod. This magazine contained more of the electric matter, and gave a more ſenſible ſpark, upon the approach of the finger than the pointed bar.

A Magazine of this kind Mr. Mazeas had in an upper room of his houſe, into which he brought the lightning, by means of a wooden pole projecting out of his window, at the extremity of which a glaſs tube, filled with roſin, received a pointed iron rod, twelve feet long. But all this while the electrics, which they made uſe of to ſupport theſe iron rods, were too much expoſed to the open air, and conſequently were liable to be wet, which would infallibly defeat their experiments.

The moſt accurate experiments, made with theſe imperfect inſtruments, were thoſe of Mr. Monnier. He was convinced that the high ſituation in which the bar of iron had commonly been placed was not abſolutely neceſſary for this purpoſe: for he obſerved a common ſpeaking-trumpet, ſuſpended upon ſilk five or ſix feet from the ground, to exhibit very evident ſigns of electricity. He alſo found that a man placed upon cakes of roſin, and holding in his hand a wooden pole, about eighteen feet long, about which an iron wire was twiſted, was ſo well electrified when it thundered, that very lively ſparks were drawn from him; and that another man, ſtanding upon non-electrics, in the middle of a garden, and only holding up one of his hands in the air, attracted, with the other hand, ſhavings of wood which were held to him.

He ſays, that he obſerved a continual diminution of the electricity when the rain came on, though the thunder was ſtill very loud, and though the cake of roſin which ſupported his conductor was not wet. But he afterwards found, that this was not univerſally true.

He once obſerved, that when the conducting wire was ſurrounded with drops of rain, only ſome of them were electrified, as was evident from the conical figure they had, while the reſt remained round as before. It was alſo perceived, that the electrified and non-electrified drops generally ſucceeded each other alternately; which made Mr. Monnier call to mind a ſingular phenomenon, which happened ſome years before to five peaſants who were paſſing through a corn field, near Frankfort upon the Oder, during a thunder ſtorm; when the lightning killed the firſt, the third, and the fifth of them, without injuring the ſecond or the fourth *.

It was not owing to any want of attention to this ſubject that the Engliſh philoſophers were not the firſt to verify the theory of Dr. Franklin. They happened to have few opportunities of trying experiments. In the few they had, they were diſappointed by the rain wetting their apparatus, which was not better conſtructed than that of the French.

At length ſucceſs crowned the aſſiduity and happier contrivance of Mr. Canton, who, to the lower end of his conducting wire, had had the precaution to faſten a tin cover, to keep the rain from the glaſs tube which ſupported it. By this means, on the 20th of July 1752, he got ſparks at the diſtance of half an inch, but the whole appearance ceaſed in the ſpace of two minutes †.

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* Phil. Tranſ. Vol. xlvii. p. 551.

† Ibid. p. 568.

MR. WILSON, who took great pains in theſe purſuits, as he did in every thing elſe relating to electricity, perceived ſeveral electrical ſnaps, on the 12th of Auguſt following, from no other apparatus than an iron curtain rod; one end of which he put into the neck of a glaſs phial, which he held in his hand, while the other was made to point into the air.

DR. BEVIS alſo, at this time, viz. on the 12th of Auguſt, obſerved nearly the ſame appearances that Mr. Canton had done before *.

MR. CANTON afterwards, reſuming his obſervations on lightning, with the aſſiduity and accuracy with which he obſerves every thing, found, by a great number of experiments, that ſome clouds were in a poſitive, and ſome in a negative ſtate of electricity. And that, by this means, the electricity of his conductor would ſometimes change from one ſtate to the other five or ſix times in leſs than half an hour †.

THIS obſervation of Mr. Canton, on the different electricity of the clouds was made, and the account of it publiſhed in England, before it was known that Dr. Franklin had made the ſame diſcovery in America.

WHEN the air was dry, he obſerved, that the apparatus would continue electrified for ten minutes, or a quarter of an hour after the clouds had paſſed the zenith, and ſometimes till they were more than half way towards the horizon; that rain, eſpecially when the drops were large, generally brought down the electric fire; and hail in ſummer, he believed, never failed. The laſt obſervation he had made before the time of his writing this paper, when his apparatus had been electrified by a fall of thawing ſnow. This was on the 12th of November 1753, which, he ſays, was the 26th day, and the ſixty-firſt time, it had been electrified

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* Phil. Tranſ. Vol. xlvii. p. 569.

† Ibid. Vol. xlviii. pt. i. p. 356.

trified ſince it was firſt ſet up, viz. about the middle of the preceding May.

ONLY two thunder ſtorms had happened at London during that whole ſummer, and Mr. Canton's apparatus was ſo ſtrongly electrified by one of them, that the ringing of the bells (which he ſuſpended to his apparatus, to ſignify when the electrification was begun, and which were frequently rung ſo loud as to be heard in every room in the houſe) was ſtifled by the almoſt conſtant ſtream of denſe electric fire between each bell and the braſs ball, which would not ſuffer it to ſtrike.

UPON a farther occaſion, he obſerves, that in the ſucceeding months of January, February, and March, his apparatus was electrified no leſs than twenty-five times, both poſitively and negatively, by ſnow as well as by hail and rain; and almoſt to as great a degree when Fahrenheit's thermometer was between twenty-eight and thirty-four, as he had ever known it in ſummer, except in a thunder-ſtorm *.

MR. CANTON concludes his paper with propoſing the two following queries. 1. May not air ſuddenly rarefied give electric fire to, and air ſuddenly condenſed receive electric fire from clouds and vapours paſſing through it? 2. Is not the aurora borealis the flaſhing of electric fire from poſitive towards negative clouds at a great diſtance, through the upper part of the atmoſphere, where the reſiſtance is leaſt †?

MR. CANTON not only obſerved the different ſtates of poſitive and negative electricity in the clouds, but alſo noted the proportion that the one bore to the other for a conſiderable time. In the firſt period he had obſerved the clouds had been poſitively electrical 83 times, and negative 101. In this period he had punctually ſet down how often the powers had ſhifted, and the

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* Phil. Tranſ. Vol. xlviii. pt. ii. p. 785.

† Ibid. pt. i. p. 358.

whole time that the apparatus continued to be electrified, but he had intirely neglected to note the time that each power laſted. But this laſt circumſtance he afterwards carefully attended to for about two months, viz. from the 28th of June to the 23d of Auguſt 1754; and found the apparatus to be electrified poſitively thirty-one times, which taken together laſted three hours thirty-five minutes; and negatively forty-five times, the whole duration of which was ten hours thirty-nine minutes. He alſo obſerved that the poſitive power was generally the ſtronger. This account he wrote the 31ſt of Auguſt 1754.

THESE obſervations, which Mr. Canton gives me leave to make public, are extremely curious, and muſt have required great attention; but they are hardly ſufficient to authorize any general concluſion.

ONE of the effects of lightning and electricity is the melting of metals. This was firſt thought to be a cold fuſion; but that opinion is refuted, in a very ſenſible manner, by Dr. Knight, in a paper read at the Royal Society, November 22d, 1759. He obſerves, that the inſtances moſt generally given of cold fuſion are two, viz. that of a ſword being melted in its ſcabbard, and that of money being melted in a bag, both the ſcabbard and the bag remaining unhurt.

A GREAT number of authors, he ſays, have mentioned both the facts, but without giving either their own teſtimony, or that of any one elſe for the truth of them, or deſcribing any of the other concomitant circumſtances. And it ſeemed to him very poſſible, that lightning might produce effects ſimilar to thoſe above mentioned, without our being obliged to have recourſe to a cold fuſion to account for them.

IF, ſays he, the edge or external ſurface of a ſword had been melted, whilſt the main part of the blade remained entire, it would have afforded ſufficient ground to aſſert, in general terms, that the ſword was melted, and yet the ſcabbard might have remained unhurt; becauſe either the edge or ſurface of a ſword might be inſtantly melted by lightning and cooled ſo ſuddenly, as to make no impreſſion of burning upon the ſcabbard. Metals, as well as other bodies, he obſerves, will both heat and cool ſooner in proportion as they are thin and ſlender; that very ſmall wire will inſtantly become red-hot, and even melt, and run into a round globule in the flame of a common candle; though it is no ſooner removed out of the flame, but it is inſtantly cold. He therefore concludes, that the edge of a ſword, or even its ſurface, might be inſtantly melted by lightning; and being in contact with, or rather ſtill united to the reſt of the blade, which might be cold, it would part with its heat too ſuddenly to produce any appearance of burning.

He was confirmed in this reaſoning, by examining ſome fragments and particles of wire melted by lightning, which were ſent him by Mr. Mountaine. Amongſt them appeared globules of various ſizes, which had undergone very different degrees of fuſion. The largeſt of them had not been fluid enough to put on a ſperical figure, but they approached nearer to it, in proportion as they were ſmaller; ſo that, in the ſmalleſt granulæ, the fuſion was moſt perfect, the globules being very round and ſmooth. Their ſizes continued diminiſhing till they became inviſible to the naked eye, and ſome of them, when viewed with a microſcope, required a third or fourth magnifier to ſee them diſtinctly.

Some of the bits of wire were rough and ſcaly, like burnt iron, and were ſwelled in thoſe places where they were beginning to melt. Others continued ſtraight, and of an equable thickneſs; but their outward ſurface ſeemed to have undergone a perfect fuſion, ſo that there were two or more pieces adhering together, as if joined by a thin ſolder.

In the Philoſophical Tranſactions, Dr. Knight ſays, there are two or three relations which ſeemed at firſt to favour a cold fuſion, but when duly conſidered prove nothing concluſive *.

But that there is really no ſuch thing as cold fuſion, either by electricity or lightning, was moſt clearly demonſtrated by Mr. Kinnerſley, in a letter to Dr. Franklin, dated Philadelphia, March 12th, 1761.

He ſuſpended a piece of ſmall braſs wire, about twenty-four inches long, with a pound weight at the lower end; and, by ſending through it the charge of a caſe of bottles, containing above thirty feet of coated glaſs, he diſcovered what he calls a new method of wire-drawing. The wire was red-hot, the whole length well annealed, and above an inch longer than before. A ſecond charge melted it ſo that it parted near the middle, and meaſured, when the ends were put together, four inches longer than at firſt.

This experiment, he ſays, was propoſed to him by Dr. Franklin, in order to find whether the electricity, in paſſing through the wire, would ſo relax the coheſion of its conſtituent particles, as that the weight might produce a ſeparation; but neither of them had the leaſt ſuſpicion that any heat would be produced.

That he might have no doubt of the wire being *hot*, as well as *red*, he repeated the experiment on another piece of the ſame wire, encompaſſed with a gooſe quill, filled with looſe grains of gun-powder; which took fire, as readily as if it had been touched with a red-hot poker. Alſo tinder, tied to another piece of the wire, kindled by it; but when he tried a wire about twice as big, he could produce no ſuch effects.

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* Phil. Tranſ. Vol. li. pt. i. p. 294, &c.

Hence, ſays he, it appears, that the electric fire, though it has no ſenſible heat, when in a ſtate of reſt, will, by its violent motion, and the reſiſtance it meets with, produce heat in other bodies, when paſſing through them, provided they be ſmall enough. A great quantity will paſs through a large wire, without producing any ſenſible heat; when the ſame quantity, paſſing through a very ſmall one, being there confined to a narrower paſſage, the particles crowding cloſer together, and meeting with a greater reſiſtance, will make it red-hot, and even melt it.

Hence he concludes, that lightning does not melt metal by a cold fuſion, as Dr. Franklin and himſelf had formerly ſuppoſed; but that, when it paſſed through the blade of a ſword, if the quantity was not very great, it might heat the point ſo as to melt it, while the broadeſt and the thickeſt part might not be ſenſibly warmer than before.

When trees and houſes are ſet on fire by the dreadful quantity, which a cloud, or ſometimes the earth diſcharges, muſt not the heat, ſays he, by which the wood is firſt kindled, be generated by the lightning's violent motion through the reſiſting combuſtible matter?

If lightning, by its rapid motion, produced heat in itſelf as well as in other bodies (which Mr. Kinnerſley imagined was evident from ſome experiments made with his electrical thermometer, mentioned before) he thought that its ſometimes ſingeing the hair of animals killed by it might eaſily be account for; and that the reaſon of its not always doing ſo might be, that the quantity, though ſufficient to kill a large animal, might not be great enough, or not have met with reſiſtance enough, to become by its motion burning-hot.

We find, ſays he, that dwelling-houſes ſtruck with lightning, are ſeldom ſet on fire by it; but that when it paſſes through barns, with hay or ſtraw in them, or ſtore-houſes containing large quantities of hemp, or ſuch like matter, they ſeldom, if ever, eſcape a conflagration. This, he thought, might be owing to ſuch combuſtibles being apt to kindle with a leſs degree of heat than was neceſſary to kindle wood *.

All that was done by the French and Engliſh electricians, with reſpect to lightning and electricity, fell far ſhort of what was done by Signior Beccaria at Turin. His attention to the various ſtates of the atmoſphere, his aſſiduity in making experiments, his apparatus for making them, the extent of his views in making them, the minute exactneſs with which he has recorded them, and his judgment in applying them to a general theory, far exceeded every thing that had been done by philoſophers before him, or that has been done by any perſon ſince. And though I ſhall give conſiderable ſcope to my account of his experiments and obſervations, I ſhall be able to give my reader but a faint idea of the extent, variety, and value of his labours in this great field.

He made uſe both of kites and pointed rods, and of a great variety of both at the ſame time, and in different places. Some of the ſtrings of his kites had wires in them, and others had none. Some of them flew to a prodigious height, and others but low; and he had a great number of aſſiſtants, to note the nature, time, and degree of appearances, according as his views required.

To keep his kites conſtantly inſulated, and at the ſame time to give them more or leſs ſtring, and for many other purpoſes, he had the ſtring rolled upon a reel, which was ſupported by pillars of glaſs; and his conductor had a communication with the axis of the reel †.

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* Phil. Tranſ. Vol. liii. p. 92, &c.

† Lettere dell' elettriciſmo, p. 112.

To diſtinguiſh the poſitive and negative ſtate of the clouds, when the electricity was vigorous, with more certainty, and with more ſafety than it could be done by preſenting an excited ſtick of glaſs, or ſealing-wax to threads diverging from his conductor; he incloſed a pointed wire and a flat piece of lead oppoſite to it within a cylindrical glaſs veſſel, wrapped in paſteboard, ſo that the inſide could have no communication with the external light. Into this cover, and oppoſite to the point of the wire, he inſerted a very long tube of paſteboard; through which he could look from a conſiderable diſtance, and ſee the form of the electric light at the end of the wire; which is the ſureſt indication of its quality *.

From Signior Beccaria's extremely exact and circumſtantial account of the external appearances of thunder clouds, which he prefixes to his obſervations on their probable cauſes, I ſhall draw a general outline of the moſt remarkable particulars, in the uſual progreſs of a thunder ſtorm.

The firſt appearance of a thunder ſtorm (which generally happens when there is little or no wind) is one denſe cloud, or more, increaſing very faſt in ſize, and riſing into the higher regions of the air. The lower ſurface is black, and nearly level; but the upper finely arched, and well defined. Many of theſe clouds often ſeem piled one upon another, all arched in the ſame manner; but they keep continually uniting, ſwelling, and extending their arches.

At the time of the riſing of this cloud, the atmoſphere is generally full of a great number of ſeparate clouds, motionleſs, and of odd and whimſical ſhapes. All theſe, upon the appearance of the thunder cloud, draw towards it, and become more uniform in their ſhapes as they approach; till, coming very

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* Lettere dell' elettriciſmo, p. 107.

near the thunder cloud, their limbs mutually ſtretch towards one another; they immediately coaleſce, and together make one uniform maſs. Theſe he calls *adſcititious* clouds, from their coming in, to enlarge the ſize of the thunder cloud. But, ſometimes the thunder cloud will ſwell, and increaſe very faſt without the conjunction of any adſcititious clouds, the vapours in the atmoſphere forming themſelves into clouds wherever it paſſes. Some of the adſcititious clouds appear like white fringes, at the ſkirts of the thunder cloud, or under the body of it, but they keep continually growing darker and darker, as they approach to unite with it.

When the thunder cloud is grown to a great ſize, its lower ſurface is often ragged, particular parts being detached towards the earth, but ſtill connected with the reſt. Sometimes the lower ſurface ſwells into various large protuberances bending uniformly towards the earth. And ſometimes one whole ſide of the cloud will have an inclination to the earth, and the extremity of it will nearly touch the earth*. When the eye is under the thunder cloud, after it is grown larger, and well formed, it is ſeen to ſink lower, and to darken prodigiouſly; at the ſame time that a number of ſmall adſcititious clouds (the origin of which can never be perceived) are ſeen in a rapid motion, driving about in very uncertain directions under it. While theſe clouds are agitated with the moſt rapid motions, the rain generally falls in the greateſt plenty, and if the agitation be exceedingly great, it commonly hails †.

While the thunder cloud is ſwelling, and extending its branches over a large tract of country, the lightning is ſeen to dart from one part of it to another, and often to illuminate its whole maſs. When the cloud has acquired a ſufficient extent, the lightning ſtrikes between the cloud and the earth, in two oppoſite places, the path of the lightning lying through the whole body of the cloud and its branches. The longer this lightning continues, the rarer does the cloud grow, and the leſs dark is its appearance; till, at length, it breaks in different places, and ſhows a clear ſky. When the thunder cloud is thus diſperſed, thoſe parts which occupy the upper regions of the atmoſphere are equally ſpread, and very thin; and thoſe that are underneath are black, but thin too: and they vaniſh gradually, without being driven away by any wind *.

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* Lettere dell' elettriciſmo, p. 151.

† Ibid. p. 155.

Having ſeen what this philoſopher obſerved abroad, and in the air, let us ſee what he took notice of at his apparatus within doors. This never failed to be electrified upon every approach of a thunder cloud, or any of its branches; and the ſtream of fire from it was generally perpetual, while it was directly over the apparatus †.

That thunder clouds were ſometimes in a poſitive as well as negative ſtate of electricity, Signior Beccaria had diſcovered, before he heard of its having been obſerved by Dr. Franklin, or any other perſon ‡. The ſame cloud, in paſſing over his obſervatory, electrified his apparatus, ſometimes poſitively, and ſometimes negatively §. The electricity continued longer of the ſame kind, in proportion as the thunder cloud was ſimple, and uniform in its direction; but when the lightning changed its place, there commonly happened a change in the electricity of his apparatus. It would change ſuddenly after a very violent flaſh of lightning, but the change would be gradual when the lightning was moderate, and the progreſs of the thunder cloud ſlow.

It was an immediate inference from his obſervations of the lightning abroad, and his apparatus within, that the quantity

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* Lettere dell' elettriciſmo, p. 146. 176.

† Ibid. p. 167.

‡ Ibid. p. 138.

§ Ibid. p. 172.

of electric matter, in an uſual ſtorm of thunder, is almoſt inconceivably great, conſidering how many pointed bodies, as trees, ſpires, &c. are perpetually drawing it off, and what a prodigious quantity is repeatedly diſcharged to, or from the earth *.

After this ſummary view of appearances, I ſhall, in the ſame ſuccinct manner explain the hypotheſis by which this excellent philoſopher accounts for them, and ſome other principal and well known phenomena of thunder ſtorms.

Considering the vaſt quantity of electric fire that appears in the moſt ſimple thunder ſtorms, he thinks it impoſſible that any cloud, or number of clouds ſhould ever contain it all, ſo as either to diſcharge or receive it. Beſides, during the progreſs and increaſe of the ſtorm, though the lightning frequently ſtruck to the earth, the ſame clouds were the next moment ready to make a ſtill greater diſcharge, and his apparatus continued to be as much affected as ever. The clouds muſt, conſequently, have received at one place, the moment that a diſcharge was made from them in another †. In many caſes, the electricity of his apparatus, and conſequently of the clouds, would inſtantly change from one kind to another ſeveral times; an effect which cannot be accounted for by any ſimple diſcharge, or recruit. Both muſt have taken place in a very quick ſucceſſion ‡.

The extent of the clouds doth not leſſen this difficulty: for, be it ever ſo great, ſtill the quantity ought to be leſſened by every diſcharge: and, beſides, the points, by which the ſilent diſcharges are made, are in proportion to the extent of the clouds §. Nor is the difficulty leſſened by ſuppoſing that freſh clouds bring recruits; for beſides that the clouds are not ripe for the principal ſtorm, till all the clouds, to a great diſtance, have actually coaleſced, and formed one uniform maſs, thoſe recruits

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* Lettere dell' elettriciſmo, p. 180.

† Ibid. p. 183. 188.

‡ Ibid. p. 220.

§ Ibid. p. 185.

bear no ſort of proportion to the diſcharge, and whatever it was, it would ſoon be exhauſted.

The fact, therefore, muſt be, that the electric matter is continually darting from the clouds in one place, at the ſame time that it is diſcharged from the earth in another. And it is a neceſſary conſequence from the whole, that the clouds ſerve as conductors to convey the electric fluid from thoſe places of the earth which are overloaded with it, to thoſe which are exhauſted of it *.

To aſcertain this fact in the moſt complete manner, he propoſes that two obſervatories be fixed, about two leagues aſunder, in the uſual path of the thunder clouds; and that obſervations be made, whether the apparatus be not often poſitive at one place, when it is negative at the other †.

That great quantities of electric matter do ſometimes ruſh out of particular parts of the earth, and riſe through the air, into the higher regions of the atmoſphere, he thinks is evident from the great quantities of ſand, aſhes, and other light ſubſtances, which have often been carried up into the air, and ſcattered uniformly over a large tract of country ‡. No other known efficient cauſe of this phenomenon can be aſſigned, except the wind; and it has been obſerved when there was no wind ſtirring; and the light bodies have even been carried againſt the wind §. He ſuppoſes, therefore, that theſe light bodies are raiſed by a large quantity of electric matter iſſuing out of the earth, where it was overcharged with it, and (by that property of it which he had demonſtrated) attracting, and carrying with it every ſubſtance that could ſerve as a conductor in its paſſage. All theſe bodies, being poſſeſſed of an equal quantity of the electric fluid, will be diſperſed equally in the air, and conſequently

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* Lettere dell' elettriciſmo, p. 193.

† Ibid. p. 194.

‡ Ibid. p. 199.

§ Ibid. p. 225.

over that part of the earth where the fluid was wanting, and whither they ſerve to convey it *. Had theſe bodies been raiſed by the wind, they would have been diſperſed at random, and in heaps.

THIS comparatively rare phenomenon (but of which he had been more than once a ſpectator) he thinks exhibits both a perfect image, and a demonſtration, of the manner in which the vapours of the atmoſphere are raiſed to form thunder clouds. The ſame electric matter, wherever it iſſues, attracts to it, and carries up into the higher regions of the air, the watery particles that are diſperſed in the atmoſphere. The electric matter aſcends to the higher regions of the atmoſphere, being ſolicited by the leſs reſiſtance it finds there than in the common maſs of the earth; which, at thoſe times, is generally very dry, and conſequently highly electric. The uniformity with which thunder clouds ſpread themſelves, and ſwell into arches, muſt be owing to their being affected by ſome cauſe which, like the electric matter, diffuſes itſelf uniformly wherever it acts, and to the reſiſtance they meet with in aſcending through the air †. As a proof of this, ſteam, riſing from an electrified eolipile, diffuſes itſelf with the ſame uniformity, and in ſimilar arches, extending itſelf towards any conducting ſubſtance ‡.

THE ſame cauſe which firſt raiſed a cloud, from vapours diſperſed in the atmoſphere, draws to it thoſe that are already formed, and continues to form new ones, till the whole collected maſs extends ſo far as to reach a part of the earth where there is a deficiency of the electric fluid §. Thither too, will thoſe clouds, replete with electricity, be ſtrongly attracted, and there will the electric matter diſcharge itſelf upon the earth. A channel of communication being, in this manner, found, a

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* Lettere dell' elettriciſmo, p. 202.

† Ibid. p. 205.

‡ Ibid. p. 206.

§ Ibid. p. 212.

freſh ſupply of electric matter will be raiſed from the overloaded part, and will continue to be conveyed by the medium of the clouds, till the equilibrium of the fluid between the two places of the earth be reſtored. When the clouds are attracted in their paſſage by thoſe parts of the earth where there is a deficiency of the fluid, thoſe detached fragments are formed, and alſo thoſe uniform depending protuberances, which will be ſhown to be, in ſome caſes, the cauſe of water-ſpouts and hurricanes *.

THAT the electric matter, which forms and animates the thunder clouds, iſſues from places far below the ſurface of the earth, and that it buries itſelf there, is probable from the deep holes that have, in many places, been made by lightning †. Flaſhes of lightning have, alſo, been ſeen to ariſe from ſubterraneous cavities, and from wells ‡. Violent inundations have accompanied thunder ſtorms, not occaſioned by rain, but by water burſting from the bowels of the earth, from which it muſt have been diſlodged by ſome internal concuſſion. Deep wells have been known to fill faſter in thunder ſtorms §, and others have conſtantly grown turbid at the approach of thunder ‖.

THIS very riſe, as well as the whole progreſs of thunder clouds, has ſometimes been, in a manner, viſible. Exhalations have been frequently ſeen to riſe from particular caverns, attended with a rumbling noiſe, and to aſcend into the higher regions of the air, with all the phenomena of thunder ſtorms deſcribed above, according to the deſcription of perſons who lived long before the connection between electricity and lightning was ſuſpected ¶.

THE greateſt difficulty attending this theory of the origin of thunder ſtorms relates to the collection, and inſulation of electric

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* Lettere dell' elettriciſmo, p. 214. † Ibid. p. 227. ‡ Ibid. p. 228.
§ Ibid. p. 233. ‖ Ibid. p. 360. ¶ Ibid. p. 231.

matter within the body of the earth. With reſpect to the former, he has nothing particular to ſay. Some operations in nature are certainly attended with a loſs of the equilibrium in the electric fluid, but no perſon has yet aſſigned a more probable cauſe of the redundancy of the electric matter which, in fact, often abounds in the clouds, than what we may ſuppoſe poſſible to take place in the bowels of the earth. And ſuppoſing the loſs of the equilibrium poſſible, the ſame cauſe that produced the effect would prevent the reſtoring of it; ſo that not being able to force a way, at leaſt one ſufficiently ready, through the body of the earth, it would iſſue at the moſt convenient vent into the higher regions of the air, as the better paſſage. His electrical apparatus, though communicating with the earth, has frequently, in violent thunder ſtorms, given evident ſparks to his finger *.

In the enumeration of the effects of thunder ſtorms, he obſerves that a wind always blows from the place from which the thunder cloud proceeds; that this is agreeable to the obſervations of all mariners, and that the wind is more or leſs violent in proportion to the ſuddenneſs of the appearance of the thunder cloud, the rapidity of its expanſion, and the velocity with which the adſcititious clouds join it. The ſudden condenſation of ſuch a prodigious quantity of vapours muſt diſplace the air, and repel it on all ſides †.

He, in ſome meaſure, imitated even this effect of thunder, at leaſt produced a circulation of all the air in his room, by the continued electrification of his chain ‡.

Among other effects of lightning, he mentions the caſe of a man rendered exceedingly ſtiff, preſently after he was ſtruck dead in a ſtorm of thunder. But the moſt remarkable circumſtance, in this caſe, was the lightning (chuſing the beſt conductor) having

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* Lettere dell' elettriciſmo, p. 236.

† Ibid. p. 339, 340.

‡ Ibid. p. 343.

ſtruck one particular vein, near his neck, and followed it through its minuteſt ramifications; ſo that the figure of it appeared through the ſkin, finer than any pencil could have drawn it [*].

HE cautions perſons not to depend upon the neighbourhood of a higher, or, in all caſes, a better conductor than their own body; ſince, according to his repeated obſervations, the lightning by no means deſcends in one undivided track; but bodies of various kinds conduct their ſhare of it, at the ſame time, in proportion to their quantity and conducting power [†].

A GREAT number of obſervations, relating to the deſcent of lightning, confirm his theory of the manner of its aſcent: for, in many caſes, it throws before it the parts of conducting bodies, and diſtributes them along the reſiſting medium through which it muſt force its paſſage [‡].

UPON this principle it is, that the longeſt flaſhes of lightning ſeem to be made, by its forcing into its way part of the vapours in the air [§]. One of the principal reaſons why thoſe flaſhes make ſo long a rumbling, is their being occaſioned by the vaſt length of a vacuum, made by the paſſage of the electric matter. For though the air collapſes the moment after it has paſſed, and the vibration (on which the ſound depends) commences at the ſame moment, through the whole length of the track; yet, if the flaſh was directed towards the perſon who hears the report, the vibrations excited at the nearer end of the track will reach his ear much ſooner than thoſe excited at the more remote end; and the ſound will, without any repercuſſion or echo, continue till all the vibrations have ſucceſſively reached him [||].

I MUST introduce in this place a very curious experiment and obſervation of Mr. Lullin, concerning the production of electricity in the clouds. He made a long inſulated pole project from the ſide of one of the Alps; and, on the 29th of June 1766, obſerved, that when ſmall clouds of vapour, raiſed by the heat of the ſun, roſe near the foot of the mountain, and aſcended along the ſide of it; if they touched the extremity of the pole only, it was electrified; but if the whole pole, and conſequently part of the hill on which it ſtood, was likewiſe involved, it was not electrified. From this he concludes, that the electricity of the clouds is produced by their paſſing through the air while the ſun ſhines upon them. But to which of theſe two circumſtances, namely the motion through the air, or the action of the ſun's rays, this was owing, he could not determine, though he made ſeveral experiments for that purpoſe *.

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[*] Lettere dell' elettriciſmo, p. 242.

[†] Ibid. p. 246.

[‡] Ibid. p. 247.

[§] Ibid. p. 851.

[||] Ibid. p. 252.

ONE of the moſt remarkable effects of lightning is, that it gives polarity to the magnetic needle, and to all bodies that have any thing of iron in them, as bricks, &c. and by obſerving which way the poles of theſe bodies lye, it may be known, with the utmoſt certainty, in what direction the ſtroke paſſed †. In one caſe S. Beccaria actually aſcertained the direction of the lightning in this manner ‡.

SINCE a ſudden ſtroke of lightning gives polarity to magnets, he conjectures that a regular and conſtant circulation of the whole maſs of the fluid, from North to South, may be the original cauſe of magnetiſm in general §. This is a truly great thought; and, if juſt, will introduce greater ſimplicity into our conceptions of the laws of nature.

THAT this etherial current is inſenſible to us, is no proof of its non-exiſtence, ſince we ourſelves are involved in it. He had ſeen birds fly ſo near a thunder cloud, as he was ſure they would not have done, if they had been affected by its atmoſphere ||.

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* Diſſertatio phyſica, p. 42.

† Lettere dell' elettriciſmo, p. 262.

‡ Ibid. p. 263.

§ Ibid. p. 268.

|| Ibid.

This current he would not ſuppoſe to ariſe from one ſource, but from ſeveral, in the northern hemiſphere of the earth. The aberration of the common center of all theſe currents from the North point may be the cauſe of the variation of the needle, the period of this declination of the center of the currents may be the period of the variation, and the obliquity with which the currents ſtrike into the earth may be the cauſe of the dipping of the needle, and alſo why bars of iron more eaſily receive the magnetic virtue in one particular direction *.

He thinks that the *Aurora Borealis* may be this electric matter performing its circulation, in ſuch a ſtate of the atmoſphere as renders it viſible, or approaching nearer to the earth than uſual. Accordingly very vivid appearances of this kind have been obſerved to occaſion a fluctuation in the magnetic needle †.

Stones and bricks ſtruck by lighning are often vitrified. He ſuppoſes that ſome ſtones in the earth, having been ſtruck in this manner, firſt gave occaſion to the vulgar opinion of the thunder bolt ‡.

Signior Beccaria was very ſenſible that heat contributes much to the phenomena of thunder, lightning, and rain; but he could not find, by any experiment, that it tended to promote electricity. He, therefore, rather thought that heat operated, in this caſe, by exhaling the moiſture of the air, and thereby cutting off the communication of the electric fluid between one place and another, particularly between the earth and the higher regions of the air, whereby its effects were more viſible §.

Having entertained my reader with the obſervations of this great Italian genius, I muſt once more conduct him to France, where he will ſee ſeveral experiments well worth his notice. In this country we have ſeen that Dr. Franklin's theory of the identity of electricity and the matter of lightning was firſt verified, and we ſhall now ſee it verified in the grandeſt and moſt conſpicuous manner.

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* Lettere dell' elettriciſmo, p. 269.

† Ibid. p. 272.

‡ Ibid. p. 263.

§ Ibid. p. 359.

THE greateſt quantity of electricity that was ever brought from the clouds, by any apparatus prepared for that purpoſe, was by Mr. De Romas, aſſeſſor to the preſideal of Nerac. This gentleman was the firſt who made uſe of a wire interwoven in the hempen cord of an electrical kite, which he made ſeven feet and a half high, and three feet wide, ſo as to have eighteen ſquare feet of ſurface. This cord was found to conduct the electricity of the clouds more powerfully than a hempen cord would do, even though it was wetted; and, being terminated by a cord of dry ſilk it enabled the obſerver (by a proper management of his apparatus) to make whatever experiments he thought proper, without danger to himſelf.

BY the help of this kite, on the 7th of June 1753, about one in the afternoon, when it was raiſed 550 feet from the ground, and had taken 780 feet of ſtring, making an angle of near forty-five degrees with the horizon; he drew ſparks from his conductor three inches long and a quarter of an inch thick, the ſnapping of which was heard about 200 paces. Whilſt he was taking theſe ſparks, he felt, as it were, a cobweb on his face, though he was above three feet from the ſtring of the kite; after which he did not think it ſafe to ſtand ſo near, and called aloud to all the company to retire, as he did himſelf about two feet.

THINKING himſelf now ſecure enough, and not being incommoded by any body very near him, he took notice of what paſſed among the clouds which were immediately over the kite; but could perceive no lightning either there or any where elſe, nor ſcarce the leaſt noiſe of thunder, and there was no rain at all. The wind was Weſt, and pretty ſtrong, which raiſed the kite 100 feet higher, at leaſt, than in the other experiments.

Afterwards, caſting his eyes on the tin tube, which was faſtened to the ſtring of the kite, and about three feet from the ground, he ſaw three ſtraws, one of which was about one foot long, a ſecond four or five inches, and the third three or four inches, all ſtanding erect, and performing a circular dance, like puppets, under the tin tube, without touching one another.

This little ſpectacle, which much delighted ſeveral of the company, laſted about a quarter of an hour; after which, ſome drops of rain falling, he again perceived the ſenſation of the cobweb on his face, and at the ſame time heard a continual ruſtling noiſe, like that of a ſmall forge bellows. This was a farther warning of the increaſe of electricity; and from the firſt inſtant that Mr. De Romas perceived the dancing ſtraws, he thought it not adviſeable to take any more ſparks even with all his precautions; and he again intreated the company to ſpread themſelves to a ſtill greater diſtance.

Immediately after this came on the laſt act of the entertainment, which Mr. De Romas acknowledged made him tremble. The longeſt ſtraw was attracted by the tin tube, upon which followed three exploſions, the noiſe of which greatly reſembled that of thunder. Some of the company compared it to the exploſion of rockets, and others to the violent craſhing of large earthen jars againſt a pavement. It is certain that it was heard into the heart of the city, notwithſtanding the various noiſes there.

The fire that was ſeen at the inſtant of the exploſion had the ſhape of a ſpindle eight inches long and five lines in diameter. But the moſt aſtoniſhing and diverting circumſtance was produced by the ſtraw, which had occaſioned the exploſion, following the ſtring of the kite. Some of the company ſaw it at forty-five or fifty fathoms diſtance, attracted and repelled alternately, with this remarkable circumſtance, that every time it was attracted by the ſtring, flaſhes of fire were ſeen, and cracks were heard, though not ſo loud as at the time of the former exploſion.

It is remarkable, that, from the time of the exploſion to the end of the experiments, no lightning at all was ſeen, nor ſcarce any thunder heard. A ſmell of ſulphur was perceived, much like that of the luminous electric effluvia iſſuing out of the end of an electrified bar of metal. Round the ſtring appeared a luminous cylinder of light, three or four inches in diameter; and this being in the day-time Mr. De Romas did not queſtion but that, if it had been in the night, that electric atmoſphere would have appeared to be four or five feet in diameter. Laſtly, after the experiments were over, a hole was diſcovered in the ground, perpendicularly under the tin tube, an inch deep, and half an inch wide, which was probably made by the large flaſhes that accompanied the exploſions.

An end was put to theſe remarkable experiments by the falling of the kite, the wind being ſhifted into the Eaſt, and rain mixed with hail coming on in great plenty. Whilſt the kite was falling, the ſtring came foul of a penthouſe; and it was no ſooner diſengaged, than the perſon who held it felt ſuch a ſtroke in his hands, and ſuch a commotion through his whole body, as obliged him inſtantly to let it go; and the ſtring, falling on the feet of ſome other perſons, gave them a ſhock alſo, though much more tolerable *.

The quantity of electric matter brought by this kite from the clouds at another time is really aſtoniſhing. On the 26th of Auguſt 1756, the ſtreams of fire iſſuing from it were obſerved to be an inch thick, and ten feet long. This amazing flaſh of lightning, the effect of which on buildings or animal bodies,

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* Gent. Magaz. for Auguſt 1756, p. 378.

would perhaps have been equally deſtructive with any that are mentioned in hiſtory, was ſafely conducted by the cord of the kite to a non-electric body placed near it, and the report was equal to that of a piſtol.

MR. ROMAS had the curioſity to place a pigeon in a cage of glaſs, in a little edifice, which he had purpoſely placed, ſo as that it ſhould be demoliſhed by the lightning brought down by his kite. The edifice was, accordingly, ſhattered to pieces, but the cage and the pigeon were not ſtruck *.

THE Abbé Nollet, how gives this account, adds, that if a ſtroke of this kind had gone through the body of Mr. De Romas, the unfortunate profeſſor Richman had not probably been the only martyr to electricity, and adviſes, that great caution be uſed in conducting ſuch dangerous experiments †.

WHEN we conſider how many ſevere ſhocks the moſt cautious and judicious electricians often receive through inadvertence, we ſhall not be ſurpriſed, that when philoſophers firſt began to collect and make experiments upon real lightning, it ſhould ſometimes have proved a little untractable in their hands, and that they were obliged to give one another frequent cautions how to proceed with it.

THE Abbé Nollet, as early as the year 1752, adviſes that theſe experiments be made with circumſpection; as he had been informed, by letters from Florence and Bologna, that thoſe who had made them there had had their curioſity more than ſatiſfied by the violent ſhocks they had ſuſtained in drawing ſparks from an iron bar electrified by thunder. One of his correſpondents informed him, that once, as he was endeavouring to faſten a ſmall chain, with a copper ball at one of its extremities, to a great chain, which communicated with the bar at the top of the build-

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* Nollet's Letters, Vol. ii. p. 239.

† Phil. Tranſ. Vol. lii. pt. i. p. 342.

ing (in order to draw off the electric ſparks by means of the oſcillations of this ball) there came a flaſh of lightning, which he did not ſee, but which affected the chain with a noiſe like that of wild fire. At that inſtant, the electricity communicated itſel fto the chain of the copper ball, and gave the obſerver ſo violent a commotion, that the ball fell out of his hands, and he was ſtruck backwards four or five paces. He had never been ſo much ſhocked by the experiment of Leyden *.

MR. ROMAS received a ſevere ſtroke when he firſt raiſed his kite: and Mr. Dalibard ſays, that Mr. Monnier, a phyſician of St. Germain en Laye, member of the Academy of Sciences at Paris, and Mr. Bertier of the Oratory at Montmorency, a correſpondent of the Academy, were both ſtruck down by ſtrokes of lightning, as they were taking ſparks from their apparatus †.

BUT the greateſt ſufferer by experiments with lightning, ſince mankind have introduced ſo dangerous a ſubject of their inquiries, was profeſſor Richman of Peterſburgh before mentioned. He was ſtruck dead, on the 6th of Auguſt 1753, by a flaſh of lightning drawn by his apparatus into his own room, as he was attending to an experiment he was making with it. There were two accounts of this fatal accident communicated to the Royal Society, one by Dr. Watſon who had it from the beſt authority ‡; and the other tranſlated from the High Dutch §. From both theſe the following is extracted.

THE profeſſor had provided himſelf with an inſtrument which he called an *electrical gnomon*, the uſe of which was to meaſure the ſtrength of electricity. It conſiſted of a rod of metal terminating in a ſmall glaſs veſſel, ino which (for what reaſon I do not know) he put ſome braſs filings. At the top of this rod, a thread was faſtened, which hung down by the ſide of the rod when it was not electrified; but when it was, it avoided the rod, and ſtood at a diſtance from it, making an angle at the place where it was faſtened. To meaſure this angle, he had the arch of a quadrant faſtened to the bottom of the iron rod.

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* Phil. Tranſ. Vol. xlviii. pt. i. p. 205.

† Dalibard's Franklin, Vol. ii. p. 129.

‡ Phil. Tranſ. Vol. xlviii. pt. ii. p. 765.

§ Ibid. Vol. xlix. pt. i. p. 61.

HE was obſerving the effect of the electricity of the clouds, at the approach of a thunder ſtorm, upon this gnomon; and, of courſe, ſtanding with his head inclined towards it, accompanied by Mr. Solokow (an engraver, whom he frequently took with him, to be a joint obſerver of his electrical experiments, in order to repreſent them the better in cuts) when this gentleman, who was ſtanding cloſe to his elbow, obſerved a globe of blue fire, as he called it, as big as his fiſt, jump from the rod of the gnomon towards the head of the profeſſor, which was, at that inſtant, at about a foot diſtance from the rod. This flaſh killed Mr. Richman, but Mr. Solokow could give no account of the particular manner in which he was immediately affected by it: for, at the ſame time that the profeſſor was ſtruck, there aroſe a ſort of ſteam, or vapour, which intirely benumbed him, and made him ſink down upon the ground; ſo that he could not remember even to have heard the clap of thunder, which was very loud.

THE globe of fire was attended with a report as loud as that of a piſtol: a wire, which brought the electricity to his metal rod, was broken to pieces, and its fragments thrown upon Mr. Solokow's cloaths. Half of the glaſs veſſel in which the rod of the gnomon ſtood was broken off, and the filings of metal that were in it were thrown about the room.

UPON examining the effects of the lightning in the profeſſor's chamber, they found the door-caſe half ſplit through, and the door torn off, and thrown into the room *. They opened a vein

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* Phil. Tranſ. Vol. xlviii. pt. ii. p. 763.

of the breathleſs body twice, but no blood followed, and endeavoured to recover ſenſation by violent chafing, but in vain. Upon turning the corpſe with the face downwards, during the rubbing, an inconſiderable quantity of blood ran out of the mouth. There appeared a red ſpot on the forehead, from which ſpirted ſome drops of blood through the pores, without wounding the ſkin. The ſhoe belonging to the left foot was burſt open, and, uncovering the foot at that place, they found a blue mark; from which it was concluded, that the electrical force of the thunder, having entered the head, made its way out again at that foot.

UPON the body, particularly on the left ſide, were ſeveral red and blue ſpots, reſembling leather ſhrunk by being burnt. Many more blue ſpots were afterwards viſible over the whole body, and in particular over the back. That upon the forehead changed to a browniſh red, but the hair of the head was not ſinged, notwithſtanding the ſpot touched ſome of it. In the place where the ſhoe was unripped, the ſtocking was intire; as was the coat every where, the waiſtcoat only being ſinged on the foreflap, where it joined the hinder; but there appeared on the back of Mr. Solokow's coat long narrow ſtreaks, as if red-hot wires had burned off the nap, and which could not be well accounted for.

WHEN the body was opened the next day, twenty-four hours after he was ſtruck, the cranium was very intire, having no fiſſure, nor croſs opening; the brain as ſound as it poſſibly could be, but the tranſparent pellicles of the windpipe were exceſſively tender, gave way, and eaſily rent. There was ſome extravaſated blood in it, as likewiſe in the cavities below the lungs; thoſe of the breaſt being quite ſound, but thoſe towards the back of a browniſh black colour, and filled with more of the above mentioned blood: otherwiſe, none of the entrails were touched; but the throat, the glands, and the thin inteſtines were all inflamed. The ſinged leather-coloured ſpots penetrated the ſkin only. Twice twenty-four hours being elapſed, the body was ſo far corrupted that it was with difficulty they got it into a coffin*.

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* Phil. Tranſ. Vol. xlix. pt. i. p. 67.

SECTION XI.

OBSERVATIONS ON THE GENERAL STATE OF ELECTRICITY IN THE ATMOSPHERE, AND ITS MORE USUAL EFFECTS.

ELECTRICIANS, after obſerving the great quantity of electric matter with which the clouds are charged during a thunder ſtorm, began to attend to the leſſer quantities of it which might be contained in the common ſtate of the atmoſphere, and the more uſual effects of this great and general agent in nature. Mr. Monnier, whoſe obſervations of the electricity of the air during a thunder ſtorm have been already mentioned, was the firſt who found that there was very often, and perhaps always, a quantity of electric matter in the atmoſphere, when there was no appearance of thunder. This he confirmed by deciſive experiments, made at St. Germain en Laye, and publiſhed in a memoir read at the Royal Academy of Sciences at Paris November the 15th, 1752 *.

BUT more accurate experiments upon the electricity of the air were made by the Abbé Mazeas, at Chateau de Maintenon, during the months of June, July, and October 1753, and communicated to the Royal Society, in a letter to Dr. Stephen Hales.

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* Phil. Tranſ. Vol. xlviii pt. i. p. 203.

The Abbé's apparatus conſiſted of an iron rod 370 feet long, raiſed ninety feet above the horizon. It came down from a very high room in the caſtle, where it was faſtened to a ſilken cord ſix feet long; and it was carried from thence to the ſteeple of the town, where it was likewiſe faſtened to another ſilken cord of eight feet long, and ſheltered from rain. And a large key was ſuſpended, by the end of this wire, in order to receive the electric fluid.

On the 17th of June, when he began his experiments, the electricity of the air was ſenſibly felt every day, from ſun riſe till ſeven or eight in the evening, except in moiſt weather, when he could perceive no ſigns of electricity. In dry weather, the wire attracted minute bodies at no greater diſtance than three or four lines. He repeated the experiment carefully every day, and conſtantly obſerved, that, in weather void of ſtorms, the electricity of a piece of ſealing-wax of two inches long was above twice as ſtrong as that of the air. This obſervation inclined him to conclude, that in weather of equal drineſs the electricity of the air was always equal.

It did not appear to him that hurricanes and tempeſts increaſed the electricity of the air, when they were not accompanied with thunder; for that, during three days of a very violent continual wind, in the month of July, he was obliged to put ſome duſt within four or five lines of the conductor, before any ſenſible attraction could be perceived.

The direction of the winds, whether Eaſt, Weſt, North, or South, made no ſenſible alteration in the electricity of the air, except when they were moiſt.

In the drieſt nights of that ſummer, he could diſcover no ſigns of electricity in the air; but it returned in the morning when the ſun began to appear above the horizon, and vaniſhed again in the evening, about half an hour after ſun-ſet.

THE ſtrongeſt common electricity of the atmoſphere, during that ſummer, was perceived in the month of July, on a very dry day, the heavens being very clear, and the ſun extremely hot. The diſtance of ten or twelve lines was then ſufficient for the approach of the duſt to the conductor, in order to ſee the particles riſe in a vertical direction, like the filings of iron on the approach of a magnet.

ON the 27th of June, at two in the afternoon, he perceived ſome ſtormy clouds riſing above the horizon, and immediately went up to his apparatus; and, having applied the duſt to the key, it was attracted with a force which increaſed in proportion as the clouds reached the zenith. When they had come nearly over the wire, the duſt was ſo impetuouſly repelled, as to be entirely ſcattered from the paper. He drew conſiderable ſparks from it, though there was neither thunder nor lightning. When the ſtormy clouds were in the zenith of his wire, he obſerved that the electricity was increaſed to ſuch a degree, that even the ſilken thread attracted light bodies at the diſtance of ſeven or eight inches.

THESE ſtormy clouds remained about two hours above the horizon, without either thunder or lightning; nor did a very heavy rain diminiſh the electricity, except about the end, when the clouds began to be diſſipated *.

MR. KINNERSLEY obſerved, that when the air was in its drieſt ſtate, there was always a conſiderable quantity of electricity in it, and which might be eaſily drawn from it. Let a perſon, he ſays, in a negative ſtate, ſtanding out of doors, in the dark, when the air is dry, hold, with his arm extended, a long ſharp needle, pointing upwards, and he will ſoon be convinced that electricity may be drawn out of the air; not indeed very plentifully, for,

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* Phil. Tranſ. Vol. xlviii. pt. i. p. 377, &c.

being a bad conductor, it ſeems loth to part with it, yet ſome will evidently be collected. The air near the perſon's body, having leſs than the natural quantity, will have none to ſpare; but his arm being extended, as above, ſome will be collected from the remoter air, and will appear luminous as it converges to the point of the needle.

Let a perſon electrified negatively, he ſays, preſent the point of a needle horizontally, to a cork ball ſuſpended by ſilk, and the ball will be attracted towards the point, till it has parted with ſo much of its natural quantity of electricity, as to be in a negative ſtate, in the ſame degree with the perſon who holds the needle; then it will recede from the point, being, as he ſuppoſes, attracted the contrary way by the electricity of greater denſity in the air behind it. But as this opinion, he pleaſantly ſays, ſeems to deviate from *electrical orthodoxy*, he would be glad to ſee theſe phenomena better accounted for by the ſuperior and more penetrating genius of his friend Dr. Franklin, to whom he is writing.

Whether the electricity in the air, in clear dry weather, be of the ſame denſity at the height of 200 or 300 yards, as near the ſurface of the earth, he thought might be ſatisfactorily determined by Dr. Franklin's old experiment of the kite.

The twine, he ſays, ſhould have throughout a very ſmall wire in it, and the ends of the wire, where the ſeveral lengths are united, ought to be tied down with a waxed thread, to prevent their acting in the manner of points.

When he wrote this letter, he had tried the experiment twice, when the air was as dry as it ever is in that country, and ſo clear, that not a cloud had been ſeen, and found the twine each time in a ſmall degree electrified poſitively *.

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* Phil. Tranſ. Vol. liii. pt. i p. 87.

THE preceding obſervations of Mr. Monnier, Mr. Mazeas, and Mr. Kinnerſley, fall far ſhort of the extent and accuracy of thoſe of Signior Beccaria; whoſe obſervations on the general ſtate of electricity in the atmoſphere I have reſerved for the laſt place of the ſection, becauſe they are the moſt conſiderable though they were all made independent of, and, many of them, prior to thoſe mentioned before.

HE obſerved that, during very high winds, his apparatus gave no ſigns of being electrified *. Indeed he found that in three different ſtates of the atmoſphere, he could find no electricity in the air. 1. In windy and clear weather. 2. When the ſky was covered with diſtinct and black clouds, that had a ſlow motion. 3. In moiſt weather, not actually raining †. In a clear ſky, when the weather was calm, he always perceived ſigns of a moderate electricity, but interrupted. In rainy weather, without lightning, his apparatus was always electrified a little time before the rain fell, and during the time of the rain, but it ceaſed to be affected a little before the rain was over.

THE higher his rods reached, or his kites flew, the ſtronger ſigns they gave of their being electrified ‡. Alſo longer ſtrings or cords, extended and inſulated in the open air, acquired electricity ſooner than thoſe which were ſhorter. A cord 1500 Paris feet long, ſtretched over the river Po, was as ſtrongly electrified during a ſhower, without thunder, as a metallic rod, to bring lightning into his houſe, had been in any thunder ſtorm §.

HAVING two rods for bringing the lightning into his houſe, 140 feet aſunder, he obſerved, that if he took a ſpark from the higher of them, the ſpark from the other, which was thirty feet lower, was at that inſtant leſſened; but, what is remarkable, is

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* Lettere dell' elettriciſmo, p. 106.

† Ibid. p. 166.

‡ Ibid. p. 114.

§ Ibid. p. 165.

that its power revived again, though he kept his hand upon the former [*].

HE imagined that the electricity communicated to the air might ſometimes furniſh ſmall ſparks to his apparatus; ſince the air parts with the electricity it has received very ſlowly, and therefore the equilibrium of the electric matter in the air will not be reſtored ſo ſoon as in the earth and clouds [†].

AMONG the effects of a moderate electricity in the atmoſphere, Signior Beccaria reckons *rain*, *hail*, and *ſnow*.

CLOUDS that bring rain, he thought, were produced in the ſame manner as thunder clouds, only by a more moderate electricity. He deſcribes them at large, and the reſemblance which all their phenomena bear to thoſe of thunder clouds is indeed very ſtriking [‡].

HE notes ſeveral circumſtances attending rain without lightning, which make it very probable, that it is produced by the ſame cauſe as when it is accompanied with lightning. Light has been ſeen among the clouds by night in rainy weather; and even by day rainy clouds are ſometimes ſeen to have a brightneſs evidently independent of the ſun [§]. The uniformity with which the clouds are ſpread, and with which the rain falls, he thought were evidences of an uniform cauſe like that of electricity [‖]. The intenſity of electricity in his apparatus generally correſponded very nearly, to the quantity of rain that fell in the ſame time [¶]. Nor is any thing to be inferred to the contrary of this ſuppoſition from the apparatus not being always electrified during rain. It has ſometimes failed during thunder. Indeed it follows from his general theory, that the electricity of his apparatus could not always correſpond to the electricity of the clouds; ſince it muſt in ſome meaſure depend upon the ſituation of the obſervatory, with reſpect to thoſe parts of the earth or clouds which are giving or taking electric fire. This was confirmed by an obſervation which he made upon one thunder cloud, which paſſed over his obſervatory. At its approach his apparatus was electrified poſitively, when it was directly over him all ſigns of electricity ceaſed, and when it was paſſed, his apparatus was electrified negatively *. This obſervation very much favours his general theory of thunder clouds.

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[*] Lettere dell' elettriciſmo, p. 176. [†] Ibid. p. 347. [‡] Ibid. p. 284.
[§] Ibid. p. 288. [‖] Ibid. p. 299. [¶] Ibid. p. 307.

SOMETIMES all the phenomena of thunder, lightning, hail, rain, ſnow, and wind, have been obſerved at one time; which ſhows the connection they all have with ſome common cauſe †.

SIGNIOR BECCARIA, therefore, ſuppoſes that, previous to rain, a quantity of electric matter eſcapes out of the earth, in ſome place where there was a redundancy of it; and, in its aſcent to the higher regions of the air, collects and conducts into its path a great quantity of vapours. The ſame cauſe that collects, will condenſe them more and more: till, in the places of the neareſt intervals, they come almoſt into contact, ſo as to form ſmall drops; which uniting with others as they fall, come down, in rain. The rain will be heavier in proportion as the electricity is more vigorous, and the cloud approaches more nearly to a thunder cloud ‡.

HE imitated the appearance of clouds that bring rain by inſulating himſelf between the rubber and conductor of his electrical machine, and with one hand dropping *colophonia* into a ſpoon faſtened to the conductor, and holding a burning coal, while his other hand communicated with the rubber. In theſe circumſtances the ſmoke ſpread along his arm, and, by degrees, all over his body, till it came to the other hand that communicated with the rubber. The lower ſurface of this ſmoke was every where parallel to his cloaths, and the upper ſurface was ſwelled and arched like clouds replete with thunder and rain *. In this manner, he ſuppoſes, the clouds that bring rain diffuſe themſelves from over thoſe parts of the earth which abound with electric fire, to thoſe parts that are exhauſted of it; and, by letting fall their rain, reſtore the equilibrium between them.

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* Lettere dell' elettriciſmo, p. 310. † Ibid. p. 290. 345. ‡ Ibid. p. 305.

Signior Beccaria thought that the electricity communicated to the air, which both receives and parts with it ſlowly, would account for the retention of vapours in a clear ſky; for ſmall diſjoined clouds, not diſperſed into rain; for the ſmaller and lighter clouds in the higher regions of the air, which are but little affected by electricity; and alſo for the darker, heavy, and ſluggiſh clouds in the lower regions, which retain more of it †. The degree of electricity which he could communicate to the air of his room, notwithſtanding its being in contact with the floor, the walls, &c. made this appear to him both poſſible and probable ‡.

He even imagined, that ſome alteration in the weight of the air might be made by this electricity of it §. He obſerved his barometer to fall a little immediately upon a flaſh of lightning; but he acknowledges that this circumſtance is no ſufficient foundation to ſuppoſe that electricity will account for *much* variation of the height of the barometer ‖. But he thought that the phenomena of rain favoured the ſuppoſition, that the electric matter in the air did, in ſome meaſure, leſſen its preſure. For when the electric matter is actually in the air, collecting and condenſing the vapours, the barometer is loweſt. When the communication is made between the earth and the clouds by the rain, the quickſilver begins to riſe; the electric matter, which ſupported part of the preſſure, being diſcharged. And this, he ſhows, will be the caſe whether the electricity in the air be poſitive or negative *.

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* Lettere dell' elettriciſmo, p. 294. † Ibid. p. 348, 349. ‡ Ibid. p. 350. § Ibid. ‖ Ibid. p. 353.

HAIL, this ingenious philoſopher ſuppoſes to be formed in the higher regions of the air, where the cold is intenſe, and where the electric matter is very copious. In theſe circumſtances, a great number of particles of water are brought near together, where they are frozen, and in their deſcent collect other particles: ſo that the denſity of the ſubſtance of the hail-ſtone grows leſs and leſs from the center; this being formed firſt, in the higher regions, and the ſurface being collected in the lower. Agreeable to this, it is obſerved, that, in mountains, hail-ſtones, as well as drops of rain, are very ſmall; there being but ſmall ſpace through which they can fall, and thereby increaſe their bulk. Drops of rain and hail agree alſo in this circumſtance, that the more intenſe is the electricity that forms them, the larger they are †. Motion is known to promote freezing, and ſo the rapid motion of the electrified clouds may promote that effect in the air ‡.

CLOUDS of ſnow differ in nothing from clouds of rain, but in the circumſtance of cold, which freezes them. Both the regular diffuſion of ſnow, and the regularity in the ſtructure of the parts of which it conſiſts (particularly ſome figures of ſnow or hail, which he calls *roſette*, and which fall about Turin) ſhow the clouds of ſnow to be actuated by ſome uniform cauſe, like electricity §. He even endeavours, very particularly, to ſhow in what manner certain configurations of ſnow are made, by the uniform action of electricity ||. All theſe conjectures about the cauſe of hail and ſnow were confirmed by obſerving, that his apparatus never failed to be electrified by ſnow, as well as by rain.

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* Lettere dell' ellettriciſmo, p. 354.

† Ibid. p. 314.

‡ Ibid. p. 318.

§ Ibid. p. 320. 322. 325.

|| Ibid. p. 325. 331. 353.

A more intenſe electricity unites the particles of hail more cloſely than the more moderate electricity does thoſe of ſnow. In like manner, we ſee thunder clouds more denſe than thoſe which merely bring rain, and the drops of rain are larger in proportion, though they often fall not from ſo great a height *.

I shall conclude this ſection with obſerving, that profeſſor Winthrop found his apparatus to be ſtrongly electrified for ſeveral hours, while the ſnow, which fell the day before (and which had not electrified his apparatus while it was falling) was driven about by a high wind. The ſame he had obſerved twice before. Franklin's Letters, new edition, p. 494.

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* Lettere dell' ellettriciſmo, p. 328.

SECTION

SECTION XII.

THE ATTEMPTS THAT HAVE BEEN MADE TO EXPLAIN SOME OF THE MORE UNUSUAL APPEARANCES IN THE EARTH AND HEAVENS BY ELECTRICITY.

IN the two preceding ſections of this period, relating to the electricity of the atmoſphere, the experiments and obſervations of Signior Beccaria have made a principal figure; and the materials I have collected from him make a no leſs conſiderable part of this. They who may have thought he indulged too much to imagination before, will think him abſolutely extravagant here; but his extravagancies, if they be ſuch, are thoſe of a great genius; and had he a thouſand more ſuch extravagancies, I ſhould, with pleaſure, have followed him through them all.

THE meteor, uſually called a *falling ſtar*, has hitherto puzzled all philoſophers. Signior Beccaria makes it pretty evident, that it is an electrical appearance; and the fact which he relates as a proof of it, is exceedingly curious and remarkable.

As he was one time ſitting with a friend in the open air, an hour after ſun-ſet, they ſaw what is called a falling ſtar directing its courſe towards them, and apparently growing larger and larger, till it diſappeared not far from them; when it left their faces, hands, and cloaths, with the earth, and all the neighbouring

ing objects, ſuddenly illuminated, with a diffuſed and lambent light, attended with no noiſe at all. While they were ſtarting up, ſtanding, and looking at one another, ſurpriſed at the appearance, a ſervant came running to them out of a neighbouring garden, and aſked them if they had ſeen nothing; for that he had ſeen a light ſhine ſuddenly in the garden, and eſpecially upon the ſtreams which he was throwing to water it*.

All theſe appearances were evidently electrical; and Signior Beccaria was confirmed in his conjecture, that electricity was the cauſe of them, by the quantity of electric matter which, as was mentioned before, he had ſeen gradually advancing towards his kite; for that, he ſays, had very much the appearance of a falling ſtar. Sometimes alſo he ſaw a kind of *glory* round the kite, which followed it when it changed its place, but left ſome light, for a ſmall ſpace of time, in the place which it had quitted†.

That appearances, which bear evident marks of electricity, have a very ſenſible progreſſive motion, is demonſtrated from a variety of meteorological obſervations. I ſhall relate one made by Mr. Chalmers, when he was on board the Montague under the command of Admiral Chambers. The account of it was read at the Royal Society, March the 22d, 1749.

On the 4th of November 1749, in lat. 42° 48′ long. 9° 3′ he was taking an obſervation on the quarter-deck, about ten minutes before twelve, when one of the quarter-maſters deſired he would look to the wind-ward; upon which he obſerved a large ball of blue fire rolling on the ſurface of the water, at about three miles diſtance from them. They immediately lowered their topſails, &c. but it came down upon them ſo faſt, that before they could raiſe the main-tack, they obſerved the ball to riſe almoſt perpendicular, and not above forty or fifty yards from the main chains; when it went off with an exploſion as if hundreds of cannon had been fired at one time, and left ſo great a ſmell of brimſtone, that the ſhip ſeemed to be nothing but ſulphur. After the noiſe was over, which, he believed, did not laſt longer than half a ſecond, they found their main top-maſt ſhattered into above a hundred pieces, and the main-maſt rent quite down to the heel. There were ſome of the ſpikes which nail the fiſh of the main-maſt drawn with ſuch force out of the maſt, and they ſtuck ſo faſt in the main-deck, that the carpenter was obliged to take an iron crow to get them out. There were five men knocked down, and one of them greatly burnt by the exploſion. They believed, that when the ball, which appeared to them to be of the bigneſs of a large mill-ſtone, roſe, it took the middle of the main top-maſt, as the head of the maſt above the hounds was not ſplintered. They had a hard gale of wind from the N. by W. to the N. N. E. for two days before the accident, with a great deal of rain and hail, and a large ſea. From the northward they had no thunder or lightning, neither before nor after the exploſion. The ball came down from the North-Eaſt, and went to the South-Weſt.

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* Lettere dell' elettriciſmo, p. 111.

† Ibid. p. 130.

THAT the *Aurora Borealis* is an electrical phenomenon was, I believe never diſputed, from the time that lightning was proved to be one. To the circumſtances of reſemblance which had before been taken notice of between this phenomenon and electricity; Signior Beccaria adds, that when the *Aurora Borealis* has extended lower than uſual into the atmoſphere, various ſounds, as of rumbling, and hiſſing, have been heard *.

MR. BERGMAN ſays, he has often obſerved the magnetic needle to be diſturbed by a high aurora borealis, but that he could never procure any electricity from them, either with pointed metallic rods, or by means of a kite *.

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* Elettriciſmo artificiale e naturale, p. 221.

Mr. Canton (beſides his conjecture, mentioned before, p. 309, that the aurora borealis may be the flaſhing of electric fire from poſitive towards negative clouds at a great diſtance, through the upper part of the atmoſphere, where the reſiſtance is leaſt) ſuppoſes that the aurora borealis, which happens at the time that the needle is diſturbed by the heat of the earth, is the electricity of the heated air above it; and this, he ſays, will appear chiefly in the northern regions, as the alteration in the heat of the air in thoſe parts will be the greateſt. This hypotheſis, he adds, will not ſeem improbable, if it be conſidered, that electricity is now known to be the cauſe of thunder and lightning, that it has been extracted from the air at the time of an aurora borealis; that the inhabitants of the northern countries obſerve the aurora to be remarkably ſtrong, when a ſudden thaw happens after ſevere cold weather; and that the curious in theſe matters are now acquainted with a ſubſtance that will, without friction, both emit and abſorb the electric fluid, only by the increaſe or diminution of its heat; meaning the tourmalin, in which he had diſcovered that property †.

In a paper, dated November the 11th, 1754, he ſays he has ſometimes known the air to be electrical in clear weather, but never at night, except when there has appeared an aurora borealis, and then but to a ſmall degree, which he had ſeveral opportunities of obſerving that year. How far poſitive and negative electricity in the air, with a proper quantity of moiſture between, to ſerve as a conductor, will account for this, and other meteors, ſometimes ſeen in a ſerene ſky, he leaves to be inquired into ‡.

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* Phil. Tranſ. Vol. lii. pt. ii. p. 485.

† Ibid. Vol. li. pt. i. p. 403.

‡ Ibid. Vol. xlviii. pt. ii. p. 784.

SIGNIOR BECCARIA takes ſome pains to ſhow that *water ſpouts* have an electrical origin. To make this more evident, he firſt deſcribes the circumſtances attending their appearance, which are the following.

THEY generally appear in calm weather. The ſea ſeems to boil, and ſend up a ſmoke under them, riſing in a hill towards the ſpout. At the ſame time, perſons who have been near them have heard a rumbling noiſe. The form of a water ſpout is that of a ſpeaking-trumpet, the wider end being in the clouds, and the narrower end towards the ſea. The ſize is various, even in the ſame ſpout. The colour is ſometimes inclining to white, and ſometimes to black. Their poſition is ſometimes perpendicular to the ſea, ſometimes oblique; and ſometimes the ſpout itſelf is in the form of a curve. Their continuance is very various, ſome diſappearing as ſoon as formed, and ſome continuing a conſiderable time. One that he had heard of continued a whole hour. But they often vaniſh, and preſently appear again in the ſame place *.

THE very ſame things that water ſpouts are at ſea are ſome kinds of *whirlwinds* and *hurricanes* by land. They have been known to tear up trees, to throw down buildings, make caverns in the earth; and, in all theſe caſes, to ſcatter earth, bricks, ſtones, timber, &c. to a great diſtance in every direction †. Great quantities of water have been left, or raiſed by them, ſo as to make a kind of deluge; and they have always been attended with a prodigious rumbling noiſe.

THAT theſe phenomena depend upon electricity cannot but appear very probable from the nature of ſeveral of them; but the conjecture is made more probable from the following additional circumſtances. They generally appear in months peculiarly ſubject to thunder ſtorms, and are commonly preceded, accompanied, or followed by lightning, rain, or hail; the previous ſtate of the air being ſimilar. Whitiſh or yellowiſh flaſhes of light have ſometimes been ſeen moving with prodigious ſwiftneſs about them. And, laſtly, the manner in which they terminate exactly reſembles what might be expected from the prolongation of one of the uniform protuberances of electrified clouds, mentioned before, towards the ſea; the water and the cloud mutually attracting one another: for they ſuddenly contract themſelves, and diſperſe almoſt at once; the cloud riſing, and the water of the ſea under it falling to its level. But the moſt remarkable circumſtance, and the moſt favourable to the ſuppoſition of their depending upon electricity is, that they have been diſperſed by preſenting to them ſharp pointed knives or ſwords. This, at leaſt, is the conſtant practice of mariners, in many parts of the world where theſe water ſpouts abound; and he was aſſured by ſeveral of them, that the method has often been undoubtedly effectual *.

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* Elettriciſmo artificiale e naturale, p. 206, &c.

† Ibid. p. 210.

The analogy between the phenomena of water ſpouts and electricity, he ſays, may be made viſible, by hanging a drop of water to a wire communicating with the prime conductor, and placing a veſſel of water under it. In theſe circumſtances, the drop aſſumes all the various appearances of a water ſpout, both in its riſe, form, and manner of diſappearing. Nothing is wanting but the ſmoke, which may require a great force of electricity to become viſible.

Mr. Wilcke alſo conſiders the water ſpout as a kind of great electrical cone, raiſed between the cloud ſtrongly electrified, and the ſea or the earth †, and he relates a very remarkable appearance which occurred to himſelf, and which ſtrongly confirms his

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* Elettriciſmo artificiale e naturale, p. 213.

† Wilcke, p. 142.

ſuppoſition. On the 20th of July 1758, at three o'clock in the afternoon, he obſerved a great quantity of duſt riſing from the ground, and covering a field, and part of the town in which he then was. There was no wind, and the duſt moved gently towards the Eaſt, where there appeared a great black cloud, which, when it was near his zenith, electrified his apparatus poſitively, and to as great a degree as ever he had obſerved it to be done by natural electricity. This cloud paſſed his zenith, and went gradually towards the Weſt, the duſt then following it, and continuing to riſe higher and higher till it compoſed a thick pillar, in the form of a ſugar-loaf, and at length ſeemed to be in contact with the cloud. At ſome diſtance from this, there came, in the ſame path, another great cloud, together with a long ſtream of ſmaller clouds, moving faſter than the preceding. Theſe clouds electrified his apparatus negatively, and when they came near the poſitive cloud, a flaſh of lightning was ſeen to dart through the cloud of duſt, the poſitive cloud, the large negative cloud, and as far as the eye could diſtinguiſh, the whole train of ſmaller negative clouds which followed it. Upon this, the negative clouds ſpread very much, and diſſolved in rain, and the air was preſently clear of all the duſt. The whole appearance laſted not above half an hour *.

To Signior Beccaria's theory of water ſpouts and hurricanes, I ſhall add a deſcription of a hurricane in the Weſt Indies, from the *Account of the European Settlements in America*, part of which is tranſcribed from the Philoſophical Tranſactions. Both were evidently written without the moſt diſtant view to any philoſophical theory, and leaſt of all that of electricity; and yet thoſe who are diſpoſed to favour this hypotheſis may perceive ſeveral circumſtances, which tend to ſtrengthen it. I need not point them out.

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* Remarks on Dr. Franklin's Letters, p. 348.

" IT is in the rainy ſeaſon, principally in the month of Auguſt,
" more rarely in July and September, that they are aſſaulted by
" *hurricanes*, the moſt terrible calamity to which they are
" ſubject from the climate. This deſtroys, at one ſtroke, the
" labour of many years, and fruſtrates the moſt exalted hopes
" of the planter; and often juſt at the moment when he thinks
" himſelf out of the reach of fortune. It is a ſudden and violent
" ſtorm of wind, rain, thunder, and lightning; attended with a
" furious ſwelling of the ſea, and ſometimes with an earthquake;
" in ſhort, with every circumſtance which the elements can
" aſſemble that is terrible and deſtructive.

" FIRST they ſee, as a prelude to the enſuing havock, whole
" fields of ſugar canes whirled into the air, and ſcattered over
" the face of the country. The ſtrongeſt trees of the foreſt are
" torn up by the roots, and driven about like ſtubble. Their
" wind-mills are ſwept away in a moment. Their works, their
" fixtures, the ponderous copper boilers and ſtills, of ſeveral hundred weight, are wrenched from the ground, and battered to
" pieces. Their houſes are no protection: the roofs are torn
" off at one blaſt, whilſt the rain, which in an hour riſes five
" feet, ruſhes in upon them with an irreſiſtible violence.

" THERE are ſigns, which the Indians of theſe iſlands taught
" our planters, by which they can prognoſticate the approach
" of a hurricane. It comes on either in the quarters, or at the
" full or change of the moon. If it will come on at the full
" moon, you being at the change, obſerve theſe ſigns. That
" day you will ſee the ſky very turbulent. You will obſerve
" the ſun more red than at other times. You will perceive a
" dead calm, and the hills clear of all thoſe clouds and miſts
" which uſually hover about them. In the clefts of the earth,

" and in the wells, you will hear a hollow rumbling ſound, like " the ruſhing of a great wind. At night the ſtars ſeem much " larger than uſual, and ſurrounded with a ſort of burs. The " North-weſt ſky has a black and menacing look, and the ſea " emits a ſtrong ſmell, and riſes into vaſt waves, often without " any wind. The wind itſelf now forſakes its uſual ſteady Eaſter- " ly ſtream, and ſhifts about to the Weſt; from whence it ſome- " times blows, with intermiſſions, violently and irregularly, " for about two hours at a time. You have the ſame ſigns at " the full of the moon. The moon itſelf is ſurrounded with a " great bur, and ſometimes the ſun has the ſame appearance *."

THE firſt perſon who advanced that *earthquakes* were probably cauſed by electricity, was Dr. STUKELEY, upon occaſion of the earthquakes at London, on February the 8th, and on March the 8th, 1749; and another which affected various other parts of England, the center being about Daventry in Northamptonſhire, on the 30th of September 1750. The papers which the Doctor delivered to the Royal Society on theſe occaſions, and which were read, March the 22d, 1749, and December the 6th, 1750, well deſerve the attention of all philoſophers and electricians. I ſhall here give the ſubſtance of both; only abridging, and differently arranging the materials of them.

THAT earthquakes are not owing to ſubterraneous winds, fires, vapours, or any thing that occaſions an exploſion, and heaves up the ground, he thought might eaſily be concluded from a variety of circumſtances. In the firſt place, he thought there was no evidence of any remarkable cavernous ſtructure of the earth; but that, on the contrary, there is rather reaſon to preſume, that it is, in a great meaſure, ſolid; ſo as to leave little room for internal changes and fermentations within its ſubſtance;

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* Account of the European Settlements in America, Vol. ii. p. 96, &c. Phil. Tranſ. abridged, Vol. ii. p. 106.

nor do coal-pits, he ſays, when on fire, ever produce any thing reſembling an earthquake.

In the ſecond earthquake at London, there was no ſuch thing as fire, vapour, ſmoke, ſmell, or an eruption of any kind obſerved, though the ſhock affected a circuit of thirty miles in diameter. This conſideration alone, of the extent of ſurface ſhaken by an earthquake, he thought was ſufficient to overthrow the ſuppoſition of its being owing to the expanſion of any ſubterraneous vapours. For it could not poſſibly be imagined, that ſo immenſe a force, as could act upon that compaſs of ground inſtantaneouſly ſhould never break the ſurface of it, ſo as to be diſcoverable to the ſight or ſmell; when ſmall fire balls, burſting in the air, have inſtantly propagated a ſulphureous ſmell all around them, to the diſtance of ſeveral miles.

Besides, the operation of this great fermentation, and production of elaſtic vapours, &c. ought to be many days in continuance, and not inſtantaneous; and the evaporation of ſuch a quantity of inflammable matter would require a long ſpace of time.

He thought that if vapours and ſubterraneous fermentations, exploſions, and eruptions were the cauſe of earthquakes, they would abſolutely ruin the whole ſyſtem of ſprings and fountains wherever they had once been: which is quite contrary to fact, even where they have been frequently repeated. Mentioning the great earthquake which happened A. D. 17, when no leſs than thirteen great cities of Aſia Minor were deſtroyed in one night, and which may be reckoned to have ſhaken a maſs of earth 300 miles in diameter, he aſks, How can we poſſibly conceive the action of any ſubterraneous vapours to produce ſuch an effect ſo inſtanteouſly? How came it to paſs, that the whole country of Aſia Minor was not at the ſame time deſtroyed, its mountains reverſed, its fountains and ſprings broken up, and ruined for ever, and the courſe of its rivers quite changed? Whereas, nothing ſuffered but the cities. There was no kind of alteration in the ſurface of the country, which, indeed, remains the ſame to this day.

To make the hypotheſis of ſubterraneous vapours, &c. being the cauſe of earthquakes the more improbable, he obſerves, that any ſubterraneous power, ſufficient to move a ſurface of earth thirty miles in diameter, as in the earthquakes which happened at London, muſt be lodged at leaſt fifteen or twenty miles below the ſurface of the earth, and therefore muſt move an inverted cone of ſolid earth, whoſe baſis is thirty miles in diameter, and axis fifteen or twenty miles; an effect which, he ſays, no natural power could produce.

UPON the ſame principle, the ſubterraneous cauſe of the earthquake in Aſia Minor muſt have moved a cone of earth of 300 miles in baſe, and 200 in the axis; which, he ſays, all the gun-powder which has ever been made ſince the invention of it would not have been able to ſtir, much leſs any vapours, which could be ſuppoſed to be generated ſo far below the ſurface.

IT is not upon the principles of any ſubterraneous exploſion that we can, in the leaſt, account for the manner in which ſhips, far from any land, are affected during an earthquake; which ſeem as if they ſtruck upon a rock, or as if ſomething thumped againſt their bottoms. Even the fiſhes are affected by an earthquake. The ſtroke, therefore, muſt be occaſioned by ſomething that could communicate motion with unſpeakably greater velocity than any heaving of the earth under the ſea, by the elaſticity of generated vapours. This could only produce a gradual ſwell, and could never give ſuch an impulſe to the water, as would make it feel like a ſtone.

COMPARING all theſe circumſtances, Dr. Stukeley ſays, he had always thought, that an earthquake was an electrical ſhock, of the ſame nature with thoſe which are now become familiar in electrical experiments. And this hypotheſis he thought was confirmed by the phenomena preceding and attending earthquakes, particularly thoſe which occaſioned this publication.

The weather, for five or ſix months before the firſt of theſe earthquakes, had been dry and warm to an extraordinary degree, the wind generally South and South-Weſt, and that without rain; ſo that the earth muſt have been in a ſtate of electricity ready for that particular vibration in which electrification conſiſts. On this account, he obſerves, that the Northern regions of the world are but little ſubject to earthquakes in compariſon with the Southern, where the warmth and dryneſs of the air, ſo neceſſary to electricity, are common. All the flat country of Lincolnſhire before the earthquake in September, though underneath it is a watery bog, yet, through the whole preceding ſummer and autumn (as they can have no natural ſprings in ſuch a level), the drought had been ſo great on the ſurface of the earth, that the inhabitants were obliged to drive their cattle ſeveral miles to water. This, he ſays, ſhows how fit the dry ſurface was for an electrical vibration; and alſo, which is of great importance, that earthquakes reach but very little below the ſurface of the earth.

Before the earthquake at London, all vegetables had been uncommonly forward. At the end of February, in that year, all ſorts of garden ſtuff, fruits, flowers, and trees were obſerved to be as forward as, in other years, about the middle of April; and electricity is well known to quicken vegetation.

The aurora borealis had been very frequent about the ſame time, and had been twice repeated juſt before the earthquake, of ſuch colours as had never been ſeen before. It had alſo removed to the South, contrary to what is common in England; ſo that ſome Italians, and people from other places where earthquakes are frequent, obſerving theſe lights, and the peculiar temperature of the air, did actually foretell the earthquake. For a fortnight before the earthquake in September, the weather was ſerene, mild, and calm; and, one evening, there was a deep red aurora borealis, covering the cope of heaven, very terrible to behold.

THE whole year had been exceedingly remarkable for fire-balls, thunder, lightning, and coruſcations, almoſt throughout all England. Fire balls were more than once ſeen in Ireland and Lincolnſhire, and particularly obſerved. And all theſe kinds of meteors, the Doctor ſays, are rightly judged to proceed from the electrical ſtate of the atmoſphere.

IN theſe previous circumſtances of the ſtate of the earth and air, nothing, he ſays, is wanting to produce the wonderful effect of an earthquake, but the touch of ſome non-electric body, which muſt neceſſarily be had *ab extra*, from the region of the air, or atmoſphere. Hence, he infers that, if a non-electric cloud diſcharge its contents upon any part of the earth in that highly electrical ſtate, an earthquake muſt neceſſarily enſue. As the diſcharge from an excited tube produces a commotion in the human body, ſo the diſcharge of electric matter from the compaſs of many miles of ſolid earth muſt needs be an earthquake; and the ſnap from the contact be the horrid uncouth noiſe attending it.

THE Doctor had been informed, by thoſe who were up and abroad the night preceding the earthquake, and early in the morning, that coruſcations in the air were extremely frequent; and that, a little before the earthquake, a large and black cloud ſuddenly covered the atmoſphere, which probably occaſioned the ſhock, by the diſcharge of a ſhower. Dr. Childrey, he ſays, obſerves, that earthquakes are always preceded by rain; and ſudden tempeſts of rain in times of great drought.

A sound was obſerved to roll from the river Thames towards Temple Bar, before the houſes ceaſed to nod, juſt as the electrical ſnap precedes the ſhock. This noiſe, an obſerver ſaid, was much greater than any he had ever heard. Others, who write upon earthquakes, commonly obſerve, that the noiſe precedes the ſhock: whereas it muſt have been quite the contrary, if the concuſſion had depended upon a ſubterraneous eruption. This noiſe attending earthquakes, the Doctor thought, could not be accounted for, but upon the principles of electricity. The earthquake in September was attended with a ruſhing noiſe, as if houſes were falling, and people, in ſome places, were ſo univerſally frighted, as to run out of their houſes, imagining that their own, and thoſe of their neighbours were tumbling on their heads. In ſome villages, the people, being at divine ſervice, were much alarmed with the noiſe; which they ſaid, beyond all compariſon, exceeded all the thunder they had ever heard.

The flames and ſulphureous ſmells, which are ſometimes obſerved during earthquakes, the Doctor thought were more eaſily accounted for, from the ſuppoſition of their being electrical phenomena, than from their being occaſioned by the eruption of any thing from the bowels of the earth.

The impreſſion made by an earthquake upon land and water, to the greateſt diſtances is, as was obſerved before, inſtantaneous, which could only be effected by electricity. In the earthquake in September, the concuſſion was felt through a ſpace of 100 miles in length, and forty in breadth; and, as far as could be judged, at the ſame inſtant of time. That this tract of ground, which amounted to 4000 ſquare miles in ſurface, ſhould be thrown into ſuch agitation in a moment, is ſuch a prodigy, the Doctor ſays, as we could never believe, or conceive, did we not know it to be fact from our ſenſes. But if we ſeek the ſolution of it, we cannot think any natural power equal to it but that of electricity, which acknowledges no ſenſible tranſition of time, no bounds.

THE little damage generally done by earthquakes, the Doctor thought to be an argument of their being occaſioned by a ſimple vibration, or tremulous motion of the ſurface of the earth, by an electrical ſnap. This vibration, he ſays, impreſſed on the water, meeting with the ſolid bottoms of ſhips and lighters, occaſions that thump which is ſaid to be felt by them: yet, of the millions of ordinary houſes, over which it paſſed, not one fell. A conſideration which ſufficiently points out what ſort of a motion this was not; alſo what ſort of a motion it was, and whence derived; not a convulſion in the bowels of the earth, but an uniform vibration along its ſurface, like that of a muſical ſtring, or what we put a drinking-glaſs into, by rubbing one's finger on the edge; which yet, being brought to a certain pitch, breaks the glaſs; undoubtedly, he adds, an electrical repulſion of its parts.

THAT earthquakes are electrical phenomena, is farther evident, he ſays, from their chiefly affecting the ſea-coaſt, places along rivers, and, I may add, eminences. The earthquake in September ſpread moſtly to the North and South, which the Doctor ſays is the direction of the Spalding river, whereby it was conveyed to the ſea ſhore, where it was particularly ſenſible; thence up Boſton channel, and ſo up Boſton river to Lincoln. The greateſt part of this earthquake diſplayed its effects along, and between the two rivers Welland and Avon, and that from their ſources down to their mouths. It likewiſe reached the river Witham, which directed the electrical ſtream that way alſo to Lincoln; for which reaſon, meeting the ſame coming from Boſton, it was moſt ſenſibly felt there. It reached, likewiſe, to the Trent at Nottingham, which conveyed it to Newark.

The firſt electrical ſtroke in this earthquake ſeemed to the Doctor to have been made on the high ground about Daventry, in Northamptonſhire. From thence it deſcended chiefly Eaſtward, and along the river Welland, from Harborough to Stamford, Spalding, and the ſea; and along the rivers Avon and Nen to Northampton, Peterborough, Wiſbich, and the ſea. It ſpread itſelf all over the vaſt level of the iſle of Ely, promoted by a great number of canals, natural and artificial, made for draining that country. It was ſtill conducted Eaſtward, by Mildenhall river in Suffolk, to Bury, and the parts adjacent. All theſe circumſtances duly conſidered were to him a confirmation of the doctrine he advanced on this ſubject.

Lastly, the Doctor adds, as a farther argument in favour of his hypotheſis, that pains in the back, rheumatic, hyſteric, and nervous caſes; head-aches, cholics, &c. were felt by many people of weak conſtitutions, for a day or two after the earthquake; juſt as they would after electrification; and, to ſome, theſe diſorders proved fatal.

In what manner the earth and atmoſphere are put into that electrical and vibratory ſtate, which prepares them to give or receive that ſnap and ſhock, which we call an earthquake, and whence it is that this electric matter comes, the Doctor does not pretend to ſay, but thinks it as difficult to account for as magnetiſm, gravitation, muſcular motion, and many other ſecrets in nature *.

To theſe obſervations of Dr. Stukeley, I ſhall add ſome circumſtances which were obſerved by Dr. Hales, in the earthquake at London, on March the 8th, 1749, as tending to ſtrengthen the hypotheſis of its being cauſed by electricity; though the Doctor, who relates them, thought that the electric appearances were only occaſioned by the great agitation which the elec-

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* Phil. Tranſ. abridged, Vol. x. p. 526. 535. and p. 541. 551.

tric fluid was put into, by the ſhock of ſo great a maſs of the earth.

At the time of the earthquake, about twenty minutes before ſix in the morning, the Doctor, being awake in bed, on a ground floor, at a houſe near the church of St. Martin's in the Fields, very ſenſibly felt his bed heave, and heard an obſcure ruſhing noiſe in the houſe, which ended in a loud exploſion up in the air, like that of a ſmall cannon. The whole duration, from the beginning to the end, ſeeming to be about four ſeconds.

This great noiſe, the Doctor conjectured, was owing to the ruſhing, or ſudden expanſion of the electric fluid at the top of St. Martin's ſpire, where all the electric effluvia, which aſcended along the large body of the tower, being ſtrongly condenſed, and accelerated at the point of the weathercock, as they ruſhed off, made ſo much the louder expanſive exploſion.

The Doctor farther ſays, that the ſoldiers, who were upon duty in St. James's park, and other perſons who were then up, ſaw a blackiſh cloud, and a conſiderable lightning, juſt before the earthquake began *.

Mr. Hartman is of opinion that electricity is the cauſe of earthquakes, and gives a ſuccinct enumeration of all the circumſtances which favour this hypotheſis †.

My reader, who has ſeen to how great an extent Signior Beccaria has already carried the principles of electricity, will have no doubt but that he ſuppoſes *earthquakes* to be derived from that cauſe. And indeed, without any knowledge of what Dr. Stukeley had done, he did ſuppoſe them to be electrical phenomena; but, contrary to the Doctor, imagined the electric matter which occaſioned them to be lodged deep in the bowels of the earth, agreeable to his hypotheſis concerning the origin of lightning.

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* Phil. Tranſ. abridged, Vol. x. p. 540, 541.

† Abhandlung, p. 148.

It is certain that if Signior Beccaria's account of the origin of thunder clouds be admitted, there will be little difficulty in admitting farther, that *earthquakes* are to be reckoned among the effects of electricity. For if the equilibrium of the electric matter can, by any means, be loſt in the bowels of the earth; ſo that the beſt method of reſtoring it ſhall be by the fluid burſting its way into the air, and traverſing ſeveral miles of the atmoſphere to come to the place where it is wanted; it may eaſily be imagined, that violent concuſſions may be given to the earth, by the ſudden paſſage of this powerful agent. And ſeveral circumſtances attending earthquakes he thought rendered this hypotheſis by no means improbable.

Volcanos are known to have a near connection with earthquakes; and flaſhes of light, exactly reſembling lightning, have frequently been ſeen to ruſh from the top of Mount Veſuvius, at the time that aſhes and other light matter have been carried out of it into the air, and been diſperſed uniformly over a large tract of country. Of theſe he produces a great number of inſtances, from the beſt authority [*].

A rumbling noiſe, like thunder, is generally heard during an earthquake. At ſuch times, alſo, flaſhes of light have been ſeen riſing out of the ground, and darting up into the air. Real lightning hath ſometimes occaſioned ſmall ſhakings of the earth, at leaſt has been attended by them. But the ſtrongeſt circumſtance of reſemblance which he obſerved is the ſame that Dr. Stukeley lays ſo much ſtreſs on, viz. the amazing ſwiftneſs with which the earth is ſhaken in earthquakes. An earthquake, ſays he, is by no means a gradual heaving, as we might have expected from other cauſes, but an inſtantaneous concuſſion, ſo that the fluidity of the water is no ſecurity againſt the blow. The very ſhips, many leagues off the coaſt, feel as if they ſtruck againſt a rock.

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[*] Lettere dell' elettriciſmo, p. 226, 362, &c.

THIS admirable philoſopher, having imitated all the great phenomena of natural electricity in his own room, doth not let the earthquake eſcape him. He ſays, that if two pieces of glaſs, incloſing a thin piece of metal, be held in the hand, while a large ſhock is ſent through them, a ſtrong vibration, or concuſſion will be felt; which ſometimes, as in Dr. Franklin's experiments, breaks them to pieces.

SIGNIOR BECCARIA thinks, that there are traces of electrical operations in the earthquake, that happened at Julian's attempt to rebuild the temple of Jeruſalem *.

THAT the electric fluid is ſometimes collected in the bowels of the earth, he thought was probable from the appearance of *ignes fatui* in mines, which ſometimes happens, and is very probably an electrical phenomenon †.

WHICH of theſe two philoſophers have advanced the more probable opinion concerning the ſeat of the electric matter which occaſions earthquakes, I ſhall not pretend to decide. I ſhall only obſerve that, perhaps, a more probable general hypotheſis than either of them may be formed out of them both. Suppoſe the electric matter to be, ſome way or other, accumulated on one part of the ſurface of the earth, and, on account of the dryneſs of the ſeaſon, not eaſily to diffuſe itſelf; it may, as Signior Beccaria ſuppoſes, force itſelf a way into the higher regions of the air, forming clouds in its paſſage out of the vapours which float in the atmoſphere, and occaſion a ſudden ſhower, which may farther promote the paſſage of the fluid. The whole ſurface, thus unloaded, will receive a concuſſion, like any other conducting ſubſtance on parting with, or receiving a quantity of the electric fluid. The ruſhing noiſe will, likewiſe, ſweep over the whole extent of the country. And, upon this ſuppoſition, alſo, the fluid, in its diſcharge from the country, will naturally follow the courſe of the rivers, and alſo take the advantage of any eminences, to facilitate its aſcent into the higher regions of the air.

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* Lettere dell' elettriciſmo, p. 363.

† Dell' elettriciſmo artificiale e naturale, p. 223.

I shall cloſe this account of the theory of lightning, and other phenomena of the atmoſphere, with an enumeration of the principal appearances of natural electricity obſerved by the ancients, and which were never underſtood before the diſcovery of Dr. Franklin. It will be very eaſy for me to do this, as I find them already collected to my hands by Dr. Watſon *.

A luminous appearance, which muſt have been of an electrical nature, is mentioned by Plutarch in his life of Lyſander. He conſidered it as a meteor.

Pliny, in his ſecond book of Natural Hiſtory, calls thoſe appearances *ſtars*, and tells us, that they ſettled not only upon the maſts, and other parts of ſhips, but alſo upon men's heads. *Exſiſtunt*, ſays that hiſtorian, *ſtellæ et in mari terriſque. Vidi nocturnis militum vigiliis inhærere pilis pro vallo fulgorem effigie ea: et antennis navigantium aliiſque navium partibus, ceu vocali quodam ſono inſiſtunt, ut volucres, ſedem ex ſede mutantes ——Geminæ autem ſalutares et proſperi curſus prænunciæ; quarum adventu, fugari diram illam ac minacem appellatamque Helenam ferunt. Et ob id Polluci et Caſtori id numen aſſignant, eoſque in mari deos invocant. Hominum quoque capiti veſpertinis horis, magno præſagio circumfulgent.* But, adds he, theſe things are *incerta ratione et in naturæ majeſtate abdita.*

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* Phil. Tranſ. Vol. xlviii. pt. i. p. 210.

“ STARS make their appearance both at land and ſea. I have ſeen a light in that form on the ſpears of ſoldiers, keeping watch by night upon the ramparts. They are ſeen alſo on the ſail-yards, and other parts of ſhips, making an audible ſound, and frequently changing their places. Two of theſe lights forebode good weather, and a proſperous voyage; and drive away *one* that appears ſingle, and wears a threatening aſpect. This the ſailors call Helen, but the *two* they call Caſtor and Pollux, and invoke them as Gods. Theſe lights do ſometimes, about the evening, reſt on men's heads, and are a great and good omen. But theſe are among the awful myſteries of nature.”

SENECA in his Natural Queſtions, chap. i. takes notice of the ſame phenomenon. *Gylippo, Syracuſas petenti viſa eſt ſtella ſupra ipſam lancem conſtitiſſe. In Romanorum caſtris viſa ſunt ardere pila, ignibus ſcilicet in illis delapſis.*

“ A STAR ſettled on the lance of Gylippus, as he was ſailing to Syracuſe: and ſpears have ſeemed to be on fire in the Roman camp.”

IN Cæſar, de Bello Africano, cap. vi. edit. Amſtel. 1686, we find them attending a violent ſtorm. *Per id tempus fere Cæſaris exercitui res accidit incredibilis auditu; nempe Virgiliarum ſigno confecto, circiter vigilia ſecunda noctis, nimbus cum ſaxea grandine ſubito eſt coortus ingens. Eadem nocte legionis V. pilorum cacumina ſua ſponte arſerunt.*

“ About that time, there was a very extraordinary appearance in the army of Cæſar. In the month of February, about the ſecond watch of the night, there ſuddenly aroſe a thick cloud, followed by a ſhower of ſtones; and the ſame night, the points of the ſpears belonging to the fifth legion ſeemed to take fire.”

LIVY, cap. xxxii. mentions two ſimilar facts. *In Sicilia militibus aliquot ſpicula, in Sardinia muro circumeunti vigilias equiti, ſcipionem, quem in manu tenuerat, arſiſſe; et litora crebris ignibus fulſiſſe.*

" The ſpears of ſome ſoldiers in Sicily, and a walking-ſtick, " which a horſeman in Sardinia was holding in his hand, ſeemed " to be on fire. The ſhores were alſo luminous with frequent " fires."

These appearances are called, both by the French and Spaniards, inhabiting the coaſts of the Mediterranean, St. Helme's, or St. Telme's fires; by the Italians the fires of St. Peter, and St. Nicholas; and are frequently taken notice of by the writers of voyages.

If ſome late accounts from France, adds the Doctor, are to be depended upon, this phenomenon has been obſerved at Plauzet for time immemorial, and Mr. Binon, the Curé of the place ſays, that for twenty-ſeven years, which he has reſided there, in great ſtorms accompanied with black clouds, and frequent lightnings, the three pointed extremities of the of the croſs of the ſteeple of that place appeared ſurrounded with a body of flame; and that when this phenomenon has been ſeen, the ſtorm was no longer to be dreaded, and calm weather returned ſoon after.

Modern hiſtory furniſhes a great number of examples of flames appearing at the extremities of pointed metallic bodies projecting into the air. Little notice was taken of theſe, while the cauſe of them was unknown; but ſince their near affinity with lightning has been diſcovered, they have been more attended to, and collected.

SECTION XIII.

OBSERVATIONS ON THE USE OF METALLIC CONDUCTORS TO SECURE BUILDINGS, &c. FROM THE EFFECTS OF LIGHTNING.

THE former ſections of this period relate chiefly to the theory of electricity. In the two next I ſhall conſider what has been done towards reducing this ſcience into practice. And, in the firſt place, I ſhall recite the obſervations that have been made reſpecting the uſe of metallic conductors, to ſecure buildings from lightning, as having the neareſt connection with the ſubject of the ſections immediately preceding.

DR. FRANKLIN's propoſal to preſerve buildings from the dreadful effects of lightning was by no means a matter of mere theory. Several ſtriking facts, which occurred within the period of which I am treating, demonſtrate its utility.

INNUMERABLE obſervations ſhow how readily metallic rods actually conduct lightning, and how ſmall a ſubſtance of metal is ſufficient to diſcharge great quantities of it. Mr. Calendrini, who afterwards applied to Dr. Watſon, to be informed of the beſt methods of ſecuring powder magazines, ſays that he himſelf was an eye-witneſs of the effect of a flaſh of lightning, where he obſerved it had ſtruck the wire of a bell, and had been completely conducted by it, from one room of a houſe to another, through a very ſmall hole in the partition. This obſervation was prior to the diſcoveries of Dr. Franklin, but was recollected and recorded afterwards*.

Dr. Franklin himſelf, in a letter to Mr. Dalibard, dated Philadelphia, June the 29th, 1755, relating what had been ſhown him of the effects of lightning on the church of Newbury in New England, obſerves, that a wire not bigger than a common knitting needle, did in fact conduct a flaſh of lightning, without injuring any part of the building as far as it went, though the force of it was ſo great, that from the termination of the wire down to the ground, the ſteeple was exceedingly rent and damaged, ſome of the ſtones, even in the foundation, being torn out, and thrown to the diſtance of twenty or thirty feet. No part of the wire, however, could be found, except about two inches at each extremity, the reſt being exploded, and its particles diſſipated in ſmoke and air, as the Doctor ſays, like gunpowder by a common fire. It had only left a black ſmutty track upon the plaiſter of the wall along which it ran, three or four inches broad, darkeſt in the middle, and fainter towards the edges. From the circumſtances of this fact it was very evident, that, had the wire been continued to the foot of the building, the whole ſhock would have been conducted without the leaſt injury to it, though the wire would have been deſtroyed †.

But the moſt complete demonſtration of the real uſe of Dr. Franklin's method of ſecuring buildings from the effects of lightning, is Mr. Kinnerſley's account of what happened to the houſe of Mr. Weſt, a merchant of Philadelphia in Penſilvania, which was guarded by an apparatus conſtructed according to the directions of Dr. Franklin. It conſiſted of an iron rod, which extended about nine feet and an half above a ſtack of chimnies, to which it was fixed. It was more than half an inch in diameter in the thickeſt part, and went tapering to the upper end, in which there was a hole that received a braſs wire about three lines thick and ten inches long, terminating in a very acute point: the lower part of the apparatus joined to an iron ſtake, driven four or five feet into the ground.

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* Phil. Tranſ. Vol. liv. pt. i. p. 203.

† Ibid. Vol. xlix. pt. i. p. 309.

MR. WEST judging, by the dreadful flaſh of lightning and inſtant crack of thunder, that the conductor had been ſtruck, got it examined; when it appeared, that the top of the pointed rod was melted, and the ſmall braſs wire reduced to ſeven inches and a half in length, with its top very blunt. The ſlendereſt part of the wire he ſuſpected, had been diſſipated in ſmoke; but ſome of it, where the wire was a little thicker, being only melted by the lightning, ſunk down (while in a fluid ſtate) and formed a rough irregular cap, lower on one ſide than on the other, round the upper end of what remained, and became intimately united with it. It is remarkable, that, notwithſtanding the iron ſtake, in which the apparatus terminated, was driven three or four feet into the ground, yet the earth did not conduct the lightning ſo faſt, but that, in thunder ſtorms the lightning would be ſeen diffuſed near the ſtake two or three yards over the pavement, though at that time very wet with rain *.

IN order to ſecure ſhips from ſuſtaining damage by lightning, Dr. Watſon, in a letter to Lord Anſon, read at the Royal Society, December the 16th, 1762, adviſes, that a rod of copper, about the thickneſs of a gooſe quill, be connected with the ſpindles and iron work of the maſt; and, being continued down to the deck, be from thence, in any convenient direction, ſo diſpoſed, as always to touch the ſea water †.

WITH reſpect to powder magazines, Dr. Watſon adviſed Mr. Calandrini above mentioned, that the apparatus to conduct

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* Phil. Tranſ. Vol. liii. pt i. p. 96.

† Ibid. Vol. lii. pt. ii. p. 633.

the lightning from them be detached from the buildings themſelves, and conveyed to the next water.

What lately happened to St. Bride's church in London is a ſufficient proof of the utility of metallic conductors for lightning. Dr. Watſon, who publiſhed an account of this fact in the Philoſophical Tranſactions, obſerves, that the lightning firſt took a weather-cock, which was fixed at the top of the ſteeple; and was conducted without injuring the metal, or any thing elſe, as low as where the large iron bar or ſpindle which ſupported it (and which came down ſeveral feet into the top of the ſteeple) terminated. There, the metallic communication ceaſing, part of the lightning exploded, cracked, and ſhattered the obeliſk, which terminated the ſpire of the ſteeple, in its whole diameter, and threw off at that place ſeveral large pieces of Portland ſtone, of which the ſteeple was built. Here it likewiſe removed a ſtone from its place, but not far enough to be thrown down. From thence the lightning ſeemed to have ruſhed upon two horizontal iron bars, which were placed within the building croſs each other, to give additional ſtrength to the obeliſk, almoſt at the baſe of it, and not much above the upper ſtory. At the end of one of theſe iron bars, on the Eaſt and North-Eaſt ſide, it exploded again, and threw off a conſiderable quantity of ſtone. Almoſt all the damage done to the ſteeple, except near the top, was confined to the Eaſt and North-Eaſt ſide, and generally, where the ends of the iron bars had been inſerted into the ſtone, or placed under it; and, in ſome places, by its violence in the ſtone, its paſſage might be traced from one iron bar to another.

It is very remarkable, that, to leſſen the quantity of ſtone in this beautiful ſteeple, cramps of iron had been employed in ſeveral parts of it; and upon theſe, ſtones of no great thickneſs had been placed, both by way of ornament, and to cover the cramped joint. In ſeveral places theſe ſtones had, on account of their covering the iron, been quite blown off, and thrown away. A great number of ſtones, ſome of them large ones, were thrown from the ſteeple, three of which fell upon the roof of the church, and did great damage to it; and one of them broke through the large timbers which formed the roof, and lodged in the gallery.

UPON the whole, the ſteeple was found, on a ſurvey, to be ſo much damaged in ſeveral of its parts, that eighty-five feet were taken down, in order to reſtore it ſubſtantially; and the manner in which this ſteeple was damaged completely indicated, as Dr. Watſon obſerves, the great danger of inſulated maſſes of metal from lightning; and, on the contrary, evinced the utility and importance of maſſes of metal continued, and properly conducted, in defending them from its direful effects. The iron and lead employed in this ſteeple, in order to ſtrengthen and preſerve it, did almoſt occaſion its deſtruction; though, after it was ſtruck by the lightning, had it not been for theſe materials keeping the remaining parts together, a great part of the ſteeple muſt have fallen.

THIS building ſuffered the more, on account of the thunder ſtorm having been preceded by ſeveral very warm days. The nights had ſcarce furniſhed any dew, the air was quite dry, and in a ſtate perfectly unfit to part with its highly accumulated electricity, without violent efforts. This great dryneſs had made the ſtones of St. Bride's ſteeple, and all other buildings under the like circumſtances, far leſs fit, than if they had been in a moiſt ſtate, to conduct the lightning, and prevent the miſchief. For, though the thunder ſtorm ended in a heavy ſhower of rain, none, except a few very large drops, fell till after the church was ſtruck. And Dr. Watſon had no doubt, but that the ſucceeding rain prevented many accidents of a ſimilar kind, by bringing down, with every drop of it, part of the electric matter, and thereby reſtoring the equilibrium between the earth and clouds.

It is frequently obſerved, he ſays, that, in attending to the apparatus for collecting the electricity of the clouds, though the ſky is much darkened, and there have been ſeveral claps of thunder, at no great diſtance, yet the apparatus will ſcarcely be affected by it; but that, as ſoon as the rain begins and falls upon ſo much of the apparatus as is placed in the open air, the bells belonging to it will ring, and the electrical ſnaps ſucceed each other in a very extraordinary manner. This, as he obſerves, demonſtrates, that every drop of rain brings down part of the electric matter of a thunder cloud and diſſipates it in the earth and water, thereby preventing the miſchiefs of its violent and ſudden exploſion. Hence when the heavens have a menacing appearance, a ſhower of rain is much to be wiſhed for.

From all theſe conſiderations, Dr. Watſon had no doubt, but that the miſchief done to St. Bride's ſteeple was owing to the efforts of the lightning, after it had poſſeſſed the apparatus of the weathercock, endeavouring to force itſelf a paſſage from thence to the iron work employed in the ſteeple. As this muſt be done *per ſaltum*, as he expreſſes it, there being no regular metallic communication, it was no wonder, when its force was vehement, that it rent every thing which was not metallic that obſtructed its eaſy paſſage; and that, in this particular inſtance, the ravages increaſed, as the lightning, to a certain diſtance, came down the ſteeple.

The Doctor adviſes that, in order to have ocular demonſtration when theſe metallic conductors do really diſcharge the lightning, they be diſcontinued for an inch or two, in ſome place convenient for obſervation; in which caſe the fire will be ſeen to jump from one extremity of the wire to the other. If any danger be apprehended from this diſcontinuance of the metallic conductor, he ſays that a looſe chain may be ready to hang on, and complete the communication *.

MR. DELAVAL, who alſo gives an account of the ſame accident, obſerved, that, in every part of the building that was damaged, the lightning had acted as an elaſtic fluid, endeavouring to expand itſelf where it was accumulated in the metal; and that the effects were exactly ſimilar to thoſe which would have been produced by gunpowder pent up in the ſame places, and exploded.

IN the ſame paper Mr. Delaval gives it as his opinion, that a wire, or very ſmall rod of metal, did not ſeem to have been a canal ſufficiently large to conduct ſo great a quantity of lightning as ſtruck this ſteeple; eſpecially if any part of it, or of the metal communicating with it, was incloſed in the ſtone work, in which caſe, he thought, the application of it would tend to increaſe its bad effects, by conducting it to parts of the building which it might otherwiſe not have reached.

UPON the whole, he thought that a conductor of metal, leſs than ſix or eight inches in breadth, and a quarter of an inch in thickneſs (or an equal quantity of metal in any other form that might be found more convenient) cannot with ſafety be depended on, where buildings are expoſed to the reception of a great quantity of lightning †.

MR. WILSON, in a paper written upon the ſame occaſion, adviſes, that pointed bars or rods of metal be avoided in all conductors of lightning.

As the lightning, he ſays, muſt viſit us ſome way or other, from neceſſity, there can be no reaſon to invite it at all; but, on the contrary, when it happens to attack our buildings, we ought only ſo to contrive our apparatus, as to be able to carry

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* Phil. Tranſ. Vol. liv. p. 201, &c.

† Ibid. p. 234.

the lightning away again, by ſuch ſuitable conductors, as will very little, if at all, promote any increaſe of its quantity.

To attain this deſirable end, in ſome degree at leaſt, he propoſes, that the ſeveral buildings remain as they are at the top; that is, without having any metal above them, either pointed or not, by way of conductor; but that on the inſide of the higheſt part of the building, within a foot or two of the top, a rounded bar of metal be fixed, and continued down along the ſide of the wall, to any kind of moiſture in the ground *.

SIGNIOR BECCARIA whoſe obſervations and experience with reſpect to lightning give a weight to his opinion ſuperior to that of any other man whatever, ſeems to think very differently from Mr. Wilſon on this ſubject. He ſays that no metallic apparatus can attract more lightning than it can conduct. And ſo far is he from thinking one conductor, rounded at the top, and a foot or two under the roof ſufficient; that if the building be of any extent, he adviſes to have ſeveral of the uſual form; that is, pointed, and higher than the building. One conductor he thought ſufficient for one tower, ſteeple, or ſhip; but he thought, two neceſſary for the wing of a building 200 feet long, one at each extremity; three for two ſuch wings, the third being fixed in the middle; and four for a ſquare palace of the ſame front, one at each corner †.

MY readers at a diſtance from London will hardly believe me, when I inform them, that the elegant ſpire which has been the ſubject of a great part of this ſection, and which has been twice damaged by lightning (for it is now very probable, that a damage it received in the year 1750, was owing to the ſame cauſe) is now repaired, without any metallic conductor, to guard it in caſe of a third ſtroke.

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* Phil. Tranſ. Vol. liv. p. 249.

† Lettere dell' elettriciſmo, p. 278.

SECTION XIV.

OF MEDICAL ELECTRICITY.

THE ſubject of medical electricity falls almoſt wholly within the period of which I am now treating. For, though ſome effects of electricity upon animal bodies had been noted by the Abbé Nollet, and a few diſeaſed perſons had ſaid they had received benefit from being electrified; yet very little had been done this way, and phyſicians had ſcarcely attended to it, till within this period; whereas electricity is now become a conſiderable article in the *materia medica*.

THE firſt account I have met with of the application of electricity to medical purpoſes is of Mr. C. Kratzenſtein, profeſſor of medicine at Halle; who, in the year 1744, cured a woman of a contracted little finger in a quarter of an hour. He alſo ſo far relieved a perſon who had two lame fingers, by once electrifying them, that he could play upon the harpſicord, which he had before been diſabled from doing. He alſo obſerved, that a man's pulſe, which had beat eighty in a ſecond before he was electrified, immediately after beat eighty-eight, and was preſently increaſed to ninety-ſix *.

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* Dantzick Memoirs, Vol. i. p. 294.

There is, another celebrated inſtance of the cure of a palſy before this period; which is that performed by Mr. Jallabert, profeſſor of philoſophy and mathematics at Geneva, on a lockſmith of the age of fifty-two *, whoſe right arm had been paralytic fifteen years, occaſioned by a blow of a hammer. He was brought to Mr. Jallabert on the 26th of December 1747, and was almoſt completely cured by the 28th of February 1748. In this interval he was frequently electrified, ſparks being taken from the arm, and ſometimes the electric ſhock ſent through it †. Mr. Jallabert's own account of this cure is very circumſtantial. But it appears from the Abbé Nollet's account of his ſecond journey to Italy, that this perſon relapſed to the condition in which Mr. Jallabert found him. See the French tranſlation of this book, vol. ii. p. 396.

The report of this cure performed at Geneva engaged Mr. Sauvages of the Academy in Montpelier to attempt the cure of paralytics, in which he had conſiderable ſucceſs. In one caſe it occaſioned a ſalivation, and in another a profuſe ſweat. Many paralytics, however, were electrified without any ſucceſs. Indeed the prodigious concourſe of patients of all kinds, which the report of theſe cures brought together, was ſo great, that few of them could be electrified, except very imperfectly. For two or three months together, twenty different patients were electrified every day. It is not ſurpriſing to find, that the neighbouring populace conſidered theſe cures as an affair of witchcraft, and that the operators were obliged to have recourſe to their prieſts to undeceive them ‡. In the courſe of theſe experiments it was found, by very accurate obſervations made with a pendulum, that electrification increaſes the circulation of the blood about one ſixth.

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* Jallabert's Experiences, 143.

† Hiſtoire, pt. iii. p. 36.

‡ Ibid. p. 97.

One of the firſt who attended to electricity in a medical way was Dr. Bohadtch a Bohemian; who, in a treatiſe upon medical electricity, communicated to the Royal Society, gave it as his opinion, after the reſult of much experience, that of all diſtempers the *hemiplegia* ſeemed to be the moſt proper object of electricity. He alſo thought it might be of uſe in intermitting fevers *.

The palſy having happened to be the firſt diſorder in which electricity gave relief, there was a conſiderable number of caſes publiſhed pretty early, in which paralytics were ſaid to have found benefit from this new method of treatment. In the year 1757, Mr. Patric Brydone performed a complete cure of a hemiplegia, and, indeed, an almoſt univerſal paralytic affection, in about three days. The patient was a woman, aged thirty-three, and the palſy was of about two years continuance †. And John Godfrey Teſke, very nearly cured a young man, of the age of twenty, of a paralytic arm, of which he had not had the leaſt uſe from the age of five years ‡.

The Abbé Nollet's experiments upon paralytics had no permanent good effect §. He obſerves, however, that, during fifteen or ſixteen years that he had electrified all ſorts of perſons, he had known no one bad effect to have ariſen from it to any of them ‖.

Dr. Hart, in a letter to Dr. Watſon, dated Salop, March the 20th, 1756, mentions a cure performed by electricity upon a woman of twenty-three years of age, whoſe hand and wriſt had for ſome time been rendered uſeleſs by a violent contraction of the muſcles. She was not ſenſible of the firſt ſhock that was given her; but, as the ſhocks were repeated, the ſenſation increaſed, till ſhe was perfectly well. She was alſo cured a ſecond time, after a relapſe occaſioned by a cold *.

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* Phil. Tranſ. Vol. xlvii. p. 351. † Ibid. Vol. li. pt. i. p. 392. ‡ Ibid. Vol. li. pt. i. p. 179. § Recherches, p. 412. ‖ Ibid. p. 416.

BUT perhaps the moſt remarkable caſe that has yet occurred of the uſe of electricity in curing a diſorder of this kind, and indeed of any that is incident to the human body was of that dreadful diſorder, an univerſal *tetanus*. It is related by Dr Watſon in the Philoſophical Tranſactions; and the account was read at the Royal Society the 10th of February 1763. The patient was a girl belonging to the Foundling hoſpital, about ſeven years of age, who was firſt ſeized with a diſorder occaſioned by the worms, and at length by an univerſal rigidity of her muſcles; ſo that her whole body felt more like that of a dead animal than a living one. She had continued in this diſmal condition above a month, and about the middle of November 1762, after all the uſual medicines had failed, Dr. Watſon began to electrify her; and continued to do it by intervals, till the end of January following; when every muſcle of her body was perfectly flexible, and ſubſervient to her will, ſo that ſhe could not only ſtand upright, but could walk, and even run like other children of her age †.

DR. EDWARD SPRY relates a complete cure which he made of a locked jaw and paralyſis, in the caſe of a girl of eighteen years of age. Small ſhocks were given to the muſcles particularly affected ‡.

THAT electricity may he hurtful, and even in ſome caſes in which analogy would lead us to promiſe ourſelves it might be of uſe, is evident from many caſes, and particularly from one related by Dr. Hart of Shrewſbury, in a letter to Dr. Watſon, which was read at the Royal Society, November the 14th, 1754.

A YOUNG girl about ſixteen, whoſe right arm was paralytic, and greatly waſted in compariſon of the other, on being electrified

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* Phil. Tranſ. Vol. xlix. pt. ii. p. 558.

† Ibid. Vol. liii. p. 10.

‡ Ibid. Vol. lvii. p. 88.

twice,

twice, became univerſally paralytic, and remained ſo above a fortnight; when the new palſy was removed by proper medicines, though the firſt diſeaſed arm remained as before.

However Dr. Hart, notwithſtanding this bad accident, had a mind to try electricity again. The girl ſubmitted to it, but after having been electrified about three or four days, ſhe became a ſecond time univerſally paralytic, and even loſt her voice, and the uſe of her tongue, ſo that it was with great difficulty ſhe could ſwallow. She was relieved of this additional palſy a ſecond time by a proper courſe of medicines, continued about four months; but was diſcharged out of the hoſpital as incurable of her firſt palſy. It is ſaid that the Doctor would have tried electricity a third time; but the girl, being more nearly concerned in the experiment than her phyſician, thought proper to decline it *.

DR. FRANKLIN's account of the effects of electricity, in the manner in which he applied it, is by no means favourable to its uſe in ſuch caſes. He ſays, in a letter to Sir John Pringle, read at the Royal Society, January the 12th, 1758, that ſome years before, when the news-papers made mention of great cures performed in Italy and Germany by electricity, a number of paralytics were brought to him, from different parts of Penſilvania and the neighbouring provinces, to be electrified, and that he performed the operation at their requeſt. His method was, firſt to place the patient in a chair, or upon an electrical ſtool, and draw a number of large ſtrong ſparks from all parts of the affected limb or ſide. He then fully charged two ſix-gallon glaſs jars, and ſent the united ſhock of them through the affected limb or limbs, repeating the ſtroke commonly three times each day.

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* Phil. Tranſ. Vol. xlviii. pt. ii. p. 786.

The firſt thing he obſerved was an immediate greater ſenſible warmth in the affected limbs, which had received the ſtroke, than in the others; and the next morning the patients uſually ſaid, that, in the night, they had felt a prickling ſenſation in the fleſh of the paralytic limbs; and would ſometimes ſhow a number of ſmall red ſpots, which they ſuppoſed were occaſioned by thoſe prickings. The limbs too were found more capable of voluntary motion, and ſeemed to receive ſtrength. A man, for inſtance, who could not, the firſt day, lift his lame hand from off his knee, would the next day raiſe it four or five inches; the third day higher; and on the fifth was able, but with a feeble languid motion, to take off his hat. Theſe appearances, the Doctor ſays, gave great ſpirits to the patients, and made them hope for a perfect cure; but he did not remember that he ever ſaw any amendment after the fifth day; which the patients perceiving, and finding the ſhocks pretty ſevere, became diſcouraged, went home, and in a ſhort time relapſed; ſo that he never knew any permanent advantage from electricity in palſies.

Perhaps, ſays he, ſome permanent advantage might be obtained, if the electric ſhocks had been accompanied with proper medicine and regimen, under the direction of a ſkilful phyſician. He thought too, that many ſmall ſhocks might have been more proper than the few great ones which he gave; ſince, in an account from Scotland, a caſe was mentioned in which 200 ſhocks from a phial were given daily, and a perfect cure had been made *.

That there is an intimate connection between the ſtate of electricity in the air and the human body, is evident from ſeveral facts, particularly a very remarkable one related by the Abbé Mazeas, in a letter to Dr. Hales. He was electrifying a perſon

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* Phil. Tranſ. Vol. l. pt. ii. p. 481.

who was ſubject to epileptic fits, by his apparatus to make obſervations upon the electricity of the common atmoſphere. At firſt this perſon bore the ſparks very well, but in two or three minutes the Abbé, perceiving his countenance to change, begged he would retire, leſt any accident ſhould happen; and he was no ſooner returned home, than his ſenſes failed him, and he was ſeized with a moſt violent fit. His convulſions were taken off with ſpirits of hartſhorn, but his reaſon did not return in an hour and an half. He went up and down the ſtairs like one who walks in his ſleep, without ſpeaking to, or knowing any perſon, ſettling his papers, taking ſnuff, and offering chairs to all who came in. When he was ſpoken to, he pronounced inarticulate words, which had no connection.

When this poor man recovered his reaſon, he fell into another fit; and his friends told the Abbé, he was more affected with that diſtemper when it thundered than at any other time; and if it ever happened, which it rarely did, that he then eſcaped, his eyes, his countenance, and the confuſion of his expreſſions, ſufficiently demonſtrated the weakneſs of his reaſon.

The next day, the Abbé learned from the perſon himſelf, that the fear of thunder was not the cauſe of his diſeaſe; but that, however, he found a fatal connection between that phenomenon and his diſtemper. He added, that when the fit ſeized him, he perceived a vapour riſing in his breaſt, with ſo much rapidity, that he loſt all his ſenſes before he could call for help*.

Mr. Wilson cured a woman of a deafneſs of ſeventeen years ſtanding. He alſo obſerves, that ſhe had a very great cold when ſhe began to be electrified; but that the inflammation ceaſed the firſt time, and the cold was quite gone when the operation had been performed again the ſecond day. But he acknowledges, that he had tried the ſame experiment upon ſix other deaf perſons without any ſucceſs *.

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* Phil. Tranſ. Vol. xlviii. pt. i. p. 383.

THE ſame perſon obſerves, that one gentleman, near ſeventy years old, could never be made to receive a ſhock except in his wriſts. He ſays that he himſelf could once have borne very great ſhocks without inconvenience, but that he could not bear them at the time that he wrote.

MEDICAL electricity is very much obliged to the labours and obſervations of Mr. Lovet, lay-clerk of the cathedral church at Worceſter, who has for many years been indefatigable in the application of electricity to a great variety of diſeaſes. His ſucceſs has been very conſiderable, and all the caſes he has publiſhed ſeem to be well authenticated.

ACCORDING to Mr. Lovet, electricity is almoſt a ſpecific in all caſes of violent pains, of however long continuance, in every part of the body; as in obſtinate head-achs, the ſciatica, the cramp, and diſorders reſembling the gout. He had no trials of the proper gout, but only on thoſe who were ſlightly attacked, and who received immediate relief.

THE tooth-ach, he ſays, is generally cured inſtantly, and he ſcarce ever remembered any one who complained of its raging a minute after the operation †.

IT has ſeldom failed, he ſays, to cure rigidities, or a waſting of the muſcles, and hyſterical diſorders, particularly if they be attended with coldneſs in the feet. According to him, it cures inflammations, it has ſtopped a mortification, cured a fiſtula lachrymalis, and diſperſed extravaſated blood ‡. He alſo ſays it has been of excellent uſe in bringing to a ſuppuration, or in diſperſing without ſuppuration, obſtinate ſwellings of various kinds, even thoſe that were ſcrophulous. In his hands it cured

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* Wilſon's Eſſay, p. 207.

† Lovet's Eſſay, p. 112.

‡ Ibid. p. 76.

the falling ſickneſs, and ſeveral kinds of fits, though the patients had been ſubject to them for many years; and one cure he mentions of a hemiplegia[*]. Laſtly, he relates a well atteſted caſe, from Mr. Floyer, ſurgeon at Dorcheſter, of a complete cure of what ſeemed to be a *gutta ſerena*. The ſame Mr. Floyer, he alſo ſays, cured with it two young women of obſtructions, one of whom had taken medicines a year to no purpoſe[†].

In the rheumatiſm, Mr. Lovet candidly confeſſes, it has failed; but he ſays it was ſeldom in the caſe of young perſons, if they were taken in time.

The manner in which electricity operated in theſe cures, Mr. Lovet imagined to be, by removing ſecret obſtructions, which are probably the cauſe of thoſe diſorders. In all his practice he never knew an inſtance of harm being done by it, and thinks that, in all the caſes in which it has done harm, the manner of adminiſtering it has been injudicious. In general, he thinks the ſhocks have been made too great. This he imagined to have been the caſe of the patient before mentioned of Dr. Hart, who was made more paralytic by electric ſhocks. Mr. Lovet adviſes to begin, in general, with ſimple electrification, eſpecially in hyſterical caſes; then to proceed to taking ſparks, and laſtly to giving moderate ſhocks, but hardly ever any that are violent, or painful.

The account of the application of electricity by Dr. Zetzel of Upſal, which may be ſeen in Mr. Lovet's treatiſe, agrees in the main with the reſult of his own practice; and where there is any difference between them, Mr. Lovet thinks there are evident marks of unfairneſs in the Swediſh account. And a ſubſequent account from Sweden mentions ſeveral cures being made in thoſe very caſes, in which Dr. Zetzel ſays that no relief was to be had from electricity.

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[*] Lovet's Eſſay, p. 101.

[†] Ibid. p. 119.

The Rev. Mr. J. Weſley has followed Mr. Lovet in the ſame uſeful courſe of medical electricity, and recommends the uſe of it to his numerous followers, and to all people. Happy it is when an aſcendency over the minds of men is employed to purpoſes favourable to the increaſe of knowledge, and to the beſt intereſts of mankind. Mr. Weſley's account of cures performed by electricity agrees very well with that of Mr. Lovet, whom he often quotes. He adds, that he has ſcarce ever known an inſtance in which ſhocks all over the body have failed to cure a quotidian or tertian ague [*]. He mentions caſes of blindneſs cured or relieved by it; and ſays that he has known hearing given by it to a man that was born deaf [†]. He mentions cures in caſes of bruiſes, running ſores, the dropſy, gravel in the kidnies (cauſing the patient to part with it) a palſy in the tongue, and laſtly in the genuine conſumption. But Mr. Boiſſer ſays it is of diſſervice in phthiſical complaints [‡].

Mr. Wesley candidly ſays, he has not known any inſtance of the cure of an hemiplegia; and though many paralytics have been helped by electricity, he ſcarcely thinks that any palſy of a year's ſtanding has been thoroughly cured by it. He aſſerts, however, that he has never yet known any perſon, man, woman, or child, ſick or well, who has found (what Mr. Wilſon ſays, that ſome perſons complained of) an unuſual pain ſome days after the ſhock. Mr. Weſley had only known that the rheumatic pains, which were afterwards perfectly cured, had increaſed on the firſt or ſecond application [§].

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[*] Weſley's Deſideratum, p. 3.

[†] Ibid. p 48.

[‡] Carmichael Tentamen inaugurale medicum de Paralyſi, p. 34. ex Act. Upſ.

[§] Weſley's Deſideratum, p. 50.

MR. WESLEY directs the ſame method of adminiſtration with Mr. Lovet. In deep hyſterical caſes, he adviſes that the patients be ſimply electrified, ſitting on cakes of roſin, at leaſt half an hour, morning and evening; when, after ſome time, ſmall ſparks may be taken from them, and afterwards ſhocks given to them, more or leſs ſtrong, as their diſorder requires; which, he ſays, has ſeldom failed of the deſired effect *.

THIS account of the medical uſe of electricity by Mr. Lovet and Mr. Weſley is certainly liable to an objection, which will always lie againſt the accounts of thoſe perſons who, not being of the faculty, cannot be ſuppoſed capable of diſtinguiſhing with accuracy either the nature of the diſorders, or the conſequences of a ſeeming cure. But, on the other hand, this very circumſtance of their ignorance of the nature of diſorders, and conſequently of the beſt method of applying electricity to them, ſupplies the ſtrongeſt argument in favour of its innocence at leaſt. If in ſuch unſkilful hands it has produced ſo much good, and ſo little harm; how much more good, and how much leſs harm would it probably have produced in more ſkilful hands.

BUT whatever weight there be in this objection againſt the laſt mentioned writers, it certainly cannot be urged againſt Antonius de Haen, one of the moſt eminent phyſicians of the preſent age; who, after ſix years uninterrupted uſe of it, reckons it among the moſt valuable aſſiſtances of the medical art; and expreſsly ſays, that though it has often been applied in vain, it has often afforded relief where no other application would have been effectual. But I ſhall recite in a ſummary manner, from his *Ratio Medendi*, the reſult of all his obſervations on this ſubject.

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* Weſley's Deſideratum, p. 56.

With reſpect to partial palſies, in particular, he ſays, it never did the leaſt harm; that one or two perſons who had received no benefit from it in ſix intire months, were yet much relieved by perſevering in the uſe of it. That ſome perſons diſcontinuing it, after having received ſome benefit, relapſed again; but afterwards, by recurring to the uſe of electricity, recovered, though more ſlowly than before. Some perſons, he ſays, were relieved who had been paralytic one, three, ſix, nine, and twelve years, and ſome longer; but that in one or two of theſe caſes, the patients had received leſs relief, and more ſlowly than was uſual in recent caſes. In ſome caſes, he ſays, a moſt unexpected benefit had been found by thoſe who had been paralytic in their tongues, eyes, fingers, and other particular limbs. A paralyſis and trembling of the limbs, from whatever cauſe it aroſe, he ſays, never failed to be relieved by it; and he relates one inſtance of a perfect cure being performed in a remarkable caſe of this nature, after receiving ten ſhocks *.

De Haen's cuſtom was to apply the operation for half an hour together at leaſt. He ſeems to have uſed gentle ſhocks, and he joined to electricity, the uſe of other remedies, which, however, would not have been effectual without it †.

St. Vitus's dance, he ſays, never failed to be cured by electricity ‡. He always obſerved it to promote a more copious diſcharge of the menſes, and to relieve in caſes of obſtruction; but, for this reaſon, he adviſes that it be not adminiſtered to women with child. He found it of uſe in ſome caſes of deafneſs, but it entirely failed in its application to a gutta ſerena, and to a ſtrumous neck §.

Lastly, he relates a remarkable caſe, communicated to him by Mr. Velſe at the Hague, of the cure of a mucous apoplexy ||.

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* Ratio Medendi, Vol. i. p. 234, 199.

† Ratio Medendi, Vol. i. p. 233.

‡ Ibid. p. 389.

§ Ibid. Vol. ii. p. 200.

|| Ibid.

To the caſes which have been mentioned occaſionally, in which harm may be apprehended from electrification, may perhaps be added the venereal diſeaſe in which Mr. Veratti adviſes, that electrification be by all means avoided *.

I SHALL conclude this account of medical electricity with obſerving, that there are two general effects of electricity on the human body, of which, it ſhould ſeem, that phyſicians might greatly avail themſelves. Theſe are, that it promotes inſenſible perſpiration, and glandular ſecretion. The former is effected by ſimple electrification, and the latter by taking ſparks from the glands, or the parts contiguous to them; on which it acts like a ſtimulus. Of the former, inſtances have been produced in the experiments of the Abbé Nollet, and a few have been given occaſionally of the latter.

To theſe I ſhall now add, that Linnæus obſerved, that when electric ſparks have been drawn from the ear, it has inſtantly promoted a more copious ſecretion of ear-wax; and that it has alſo been obſerved, that, when the eye, or the parts about the eye, have been electrified, the tears have flowed copiouſly. But the moſt remarkable caſe that I have met with, is, of its promoting the ſecretion of that matter which forms the hair; whereby hair has been actually reſtored to a part that had long been bald †.

HITHERTO electricity has been generally applied to the human body either in the method of drawing ſparks, as it is called, or of giving ſhocks. But theſe operations are both violent, and though the ſtrong concuſſion may ſuit ſome caſes, it may be of diſſervice in others, where a moderate ſimple electrification might have been of ſervice.

THE great objection to this method is the tedioufneſs and expence of the application. But an electrical machine might be

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* Carmichael Tentamen, p. 34.

† Ibid. p. 33.

contrived to go by wind or water, and a convenient room might be annexed to it; in which a floor might be raiſed upon electrics, and a perſon might ſit down, read, ſleep, or even walk about during the electrification. It were to be wiſhed, that ſome phyſician of underſtanding and ſpirit would provide himſelf with ſuch a machine and room. No harm could poſſibly be apprehended from electricity, applied in this gentle and inſenſible manner, and good effects are, at leaſt, poſſible, if not highly probable. It would certainly be more for the honour of the faculty, that the practice ſhould be introduced in this manner, than that it be left to ſome rich Valetudinarian, who may take it into his head, that ſuch an operation may be of ſervice to him.

SECTION XV.

MISCELLANEOUS EXPERIMENTS AND DISCOVERIES MADE WITHIN THIS PERIOD.

HAVING diſtributed into diſtinct ſections all the ſubjects, under which I had collected materials enow to form a ſeparate account; I have reſerved for the laſt place, thoſe ſmaller articles, which could neither with propriety be introduced under the former heads, nor were large enough to make a ſection themſelves.

IT has been a great controverſy among electricians, whether glaſs be permeable to the electric fluid. Mr. Wilſon appeared in favour of the permeability, and, in a paper read at the Royal Society, December the 6th, 1759, produced the following experiments to ſupport his opinion; notwithſtanding he, even afterwards, acknowledged, in a paper read at the Royal Society, November the 13th, 1760; that, in the Leyden experiment, Dr. Franklin had proved that the fluid did not go through the glaſs *.

HE took a very large pane of glaſs, a little warmed; and holding it upright by one edge, while the oppoſite edge reſted upon wax, he rubbed the middle part of the ſurface with his finger, and found both ſides electrified *plus* †.

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* Phil. Tranſ. Vol. li. pt. ii. p. 896.

† Ibid. Vol. li. pt. i. p. 314.

Upon this I cannot help obſerving that it ought to be ſo on Dr. Franklin's principles. If one ſide be rubbed by the finger, it acquires from the finger ſome of the electric fluid. This, being ſpread on the glaſs as far as the rubbing extended, repels an equal quantity of that contained in the other ſide of the glaſs, and drives it out on that ſide, where it ſtands as an atmoſphere, ſo that both ſides are found *plus*. If the unrubbed ſide were in contact with a conductor communicating with the earth, the electric fluid would be carried away, and then that ſide would be left *apparently* in the natural ſtate. If the electric fluid found on the unrubbed ſide was really part of that which had been communicated by and from the finger, and ſo had actually *permeated* the glaſs, it might, when conducted away, be continually replaced by freſh permeating fluid communicated in the ſame manner: But if the effect is continually diminiſhing, while the ſuppoſed cauſe, repeated, continues the ſame, there ſeems reaſon to doubt the ſuppoſed relation between that cauſe and the effect. For it appears difficult to conceive how ſome electric fluid, having paſſed through a permeable body, ſhould make it more difficult for other particles of the ſame electric fluid to follow, till, at length, none could paſs at all.

Mr. Wilson alſo ſays, that, holding the ſame pane of glaſs within two feet of the prime conductor, which was electrified *plus*, that part of the glaſs which was oppoſite to the conductor became electrified *minus* on both ſides; but, in a few minutes, the *minus* electricity diſappeared, and the *plus* continuing, diffuſed itſelf into the place of the other, ſo that now the whole was electrified *plus*.

The experiment ſo far ſucceeding, induced him to make uſe of a leſs piece of glaſs, that the whole might be electrified *minus*. Theſe advances, he ſays, led him to obſerve the power of electrifying that ſmall piece of glaſs at different diſtances.

HE exposed the ſame ſmall piece of glaſs to the prime conductor, at the diſtance of two feet, and obſerved a *minus* electricity at both ſurfaces.

As he moved the glaſs nearer, to a certain diſtance, it was more ſenſibly electrified *minus*; and after that, on moving it ſtill nearer, the *minus* appearance was leſs and leſs ſenſible; till it came within the diſtance of about one inch, and then it was electrified *plus* on both ſides.

THIS *plus* electricity in the glaſs, he found, might be changed to a *minus* again, by removing the glaſs, and holding it for a time at a greater diſtance; which he thought to be a proof of the repulſive power of that fluid *.

HAVING by him a pane of glaſs, one ſide of which was rough and the other ſmooth, he rubbed it ſlightly on one ſide; upon doing which, both ſides were electrified *minus*.

ON this I muſt alſo take the liberty to obſerve that, as the electric fluid contained in glaſs in its natural ſtate, is kept equal in both ſides by the common repulſion; if the quantity in one ſide is diminiſhed, the fluid in the other ſide, being leſs repelled, retires inward, and leaves that ſurface alſo *minus*.

SLIGHT changes, *plus* or *minus*, may be made in either ſurface, that have not ſtrength to act on the other ſide, by repulſion, or by abating repulſion, through the glaſs; and ſo *plus* electricity may be given to one ſurface, and *minus* to the other in ſome degree. Both ſides may alſo be made *plus*, and both *minus*, by rubbing, or by communication, without any neceſſity of ſuppoſing the glaſs *permeable*.

AND yet it is probable that ſome glaſs, from having a greater mixture of non-electric matters in its compoſition, may be permeable, when cold, in ſome ſmall degree, as all glaſs is found to be when warmed.

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* Phil. Tranſ. Vol. li. pt. i. p. 328.

MR. WILSON treated the other ſide of this pane of glaſs in the ſame manner, after which the *minus* electricity was changed into a *plus* on both ſides.

THOUGH Dr. Franklin was of opinion, that glaſs when cold is not permeable to electricity, he had made no experiments upon it when hot; but Mr. Kinnerſley, a friend of his, made one, which ſeemed to prove, that it was very differently affected in this reſpect, in the different ſtates of hot and cold. He found, that a coated Florence flaſk (made of very thin glaſs, and full of air bubbles) containing boiling water, could not be electrified. The electricity, he ſays, paſſed as readily through it as through metal. The charge of a three pint bottle went freely through it, without injuring the flaſk in the leaſt. When it became cold, he could charge it as before. This effect he attributed to the dilatation of the pores of glaſs by heat *.

ALL Mr. Wilſon's experiments to prove the permeability of glaſs were repeated by Mr. Bergman of Upſal; and, as he ſays, with ſucceſs †.

MR. ÆPINUS, however, was by no means ſatisfied with Mr. Wilſon's experiments concerning the permeability of glaſs; and yet he brings no other fact in anſwer to his arguments, but a very common one, which ſhows that a glaſs tube both receives and loſes its electricity very ſlowly; ſo that he only aſſerts a *difficulty*, and a *ſlowneſs* in the electric fluid paſſing through electric ſubſtances, as was mentioned before; and conſequently Mr. Wilſon ſeems to have an advantage in the controverſy: for, as he ſays, paſſing through, though ever ſo ſlowly, is a real paſſing through ‡.

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* Phil. Tranſ. Vol. liii. pt. i. p. 85.

† Ibid. Vol. lii. pt. ii. p. 485.

‡ Ibid. Vol. liii. p. 443.

MR. ÆPINUS has ſhown, by a curious experiment, that though a metallic conductor and a cork ball be both electrified poſitively, ſo as to repel one another; yet, that, if the ball be forcibly brought within two, three, or four lines of the conductor, it will be attracted by it; and that it will be repelled again, if it be forcibly puſhed beyond that limit of attraction. If the ball be confined to move within the ſame ſmall diſtance, a moderate electrification of the conductor will repel the ball to its utmoſt limit; but a ſtronger electrification of the conductor will cauſe it to be attracted. He, therefore, limits the general maxim, that bodies poſſeſſing the ſame kind of electricity repel one another; and aſſerts, that this will be the caſe, only when the quantity of electric fluid belonging to them both, as one body, is greater or leſs than that which is natural to them*. This experiment deſerves particular attention.

SIGNIOR BECCARIA, who has contributed ſo largely to ſeveral former ſections in this period, furniſhes a few articles which well deſerve a place in this.

HE thought it was evident, that the electric fluid tended to move in a right line, becauſe a longer ſpark may be taken in a direct line, from the end of a long conductor, than can be taken from the ſame place in any other direction. But he thought it was ſtill more evident, from obſerving, both in the air, and in vacuo, that, preſenting the finger, or a braſs ball, at a proper diſtance, and in a certain angle with the conductor (which experience will ſoon find) the electric ſpark will make an exact curve, to which the conductor produced will be a tangent: as if the electric matter was acted upon by two different forces, one its own acquired velocity, urging it forward in a right line; the other the attraction of the body preſented to it, which throws it out of the right line *.

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* Æpini Tentamen, p. 146.

IN his obſervations on pointed bodies, he ſays, that two pointed bodies, equally ſharp, in their approach to an electrified conductor, will appear luminous only at half the diſtance at which one of them would have done †.

THE ſame ingenious philoſopher reports a curious, but cruel experiment which he made on a live cock. He detached the belly of one of the muſcles from the thigh of the animal, leaving the extremities in their proper inſertions, and then diſcharging a ſhock through it. At the inſtant of the ſtroke, the leg was violently diſtended, and the muſcle greatly inflated; the motion beginning at the tendon, and the extenſion of it reſembling the opening of a lady's fan. No pricking with a pin could make it act ſo ſtrongly ‡.

I MUST not omit to mention, in this chapter of miſcellaneous experiments, what the Dutch writers have reported concerning the gymnotus, a fiſh peculiar to Surinam, which very much reſembles what naturaliſts relate concerning the torpedo. Mr. Muſchenbroeck ſays, the gymnotus is poſſeſſed of a kind of natural electricity, but different from the common electricity, in that perſons who touch it in water are ſhocked, and ſtunned by it, ſo as to be in danger of drowning. The fiſh has been taken, and put into a veſſel; when experiments were made upon it at leiſure; and it was found, that it might be touched with all ſafety with a ſtick of ſealing-wax; but if it was touched with the naked finger, or with a piece of metal, and eſpecially a gold ring, held in the fingers, the arm was ſhocked as high as the elbow. If it was touched with the foot, the ſenſation reached as high as the knee, and the pain was as great as if the part

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* Elettriciſmo artificiale, p. 56.

† Ibid. p. 67.

‡ Lettere dell' elettriciſmo, p. 129.

had been ſtruck with ſomething hard. This kind of electricity is the ſame by night or by day, when the wind is in every direction, when the fiſh was put in veſſels of any materials, and whether it was in water or out of water. Every part of the body of the fiſh is capable of giving this ſhock, but more eſpecially the tail. The ſenſation is the ſtrongeſt when the fiſh is in motion, and it is tranſmitted to a great diſtance; ſo that if perſons in a ſhip happen to dip their fingers or feet in the ſea, when the fiſh is ſwimming at the diſtance of fifteen feet from them they are affected by it. Other fiſhes, put into the ſame veſſel with it, preſently died; but it is itſelf killed by the lobſter. The gymnotus is found in the upper part of the river of Surinam, particularly the rocky part of it. It feeds upon all kinds of fiſh, and will even eat bread. This author propoſes as a query whether the ſenſation communicated by the torpedo does not depend upon a ſimilar electricity; ſince Monſieur Reaumur ſays, that when it is touched, the hand, arm, and ſhoulder are ſeized with a ſudden ſtupor, which laſts for ſome time; and is unlike any other ſenſation [*].

This gymnotus, I ſuppoſe, is a different fiſh from the *Anguille tremblante*, *the trembling eel*, which is alſo a native of Surinam, and lives in marſhy places, from whence it cannot be drawn, except when it is intoxicated. It cannot be touched with the hand, or with a ſtick, without feeling a terrible ſtun, which reaches as high as the ſhoulder. If it be trod upon with ſhoes, the legs and thighs are affected in a ſimilar manner. Fourteen perſons joining hands, and the firſt of them touching it with a ſtick, they were all ſhocked violently. It is conjectured that this power of giving a ſhock reſides in two muſcles, which are particularly prominent and conſpicuous [†].

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[*] Muſchenbroeck's Introd. ad philoſoph. naturalem, No. 901—909.

[†] P. Fermin's Nat. Hiſt. of Surinam, p. 59.

It is to be regretted, that none of the perſons who have made experiments on theſe fiſhes ſhould have endeavoured to aſcertain whether they were capable of exhibiting the phenomena of attraction and repulſion, or the appearance of electric light, as experiments of this kind are of principal conſequence, and muſt have been very eaſy to make.

Mr. Hamilton, profeſſor of Philoſophy at the univerſity of Dublin, made a curious experiment with a wire, five or ſix inches long, finely pointed at each end. To the middle of this wire he fitted a braſs cap, which reſted on the point of a needle communicating with the conductor. Half an inch at each extremity of this wire he bent, in oppoſite directions, perpendicular to the reſt of the wire, and in the plane of the horizon. The conſequence of electrifying this apparatus was, that the wire would turn round with very great velocity; moving, as he ſays, always in a direction contrary to that in which the electric fluid iſſues from its point, without having any conducting ſubſtance near it, except the air. He alſo obſerves, that if this wire were made to turn the contrary way, it would ſtop, and turn as before*.

The ſame experiment was alſo made by Mr. Kinnerſley of Boſton, with this addition, that he electrified the wire negatively; and obſerved, to his great ſurprize, that it ſtill turned the ſame way. This he endeavoured to account for by ſuppoſing that, in the former caſe, the points, having more electricity than the air, were attracted by it; in the latter caſe, the air, having more than the points, was attracted by them †.

It may, by ſome, be thought that this pointed wire turning the ſame way, whether it be electrified negatively or poſitively, is a proof that the electric fluid iſſues out at the points in both

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[*] Phil. Tranſ. Vol. li. pt. ii. p. 905.

[†] Ibid. Vol. liii. pt. i. p. 86.

caſes alike, and by the reaction of the air is, together with the points, driven backwards; contrary to what ought to have been the caſe if the electric fluid had really iſſued out of the point in one caſe, and entered it in the other. But it will be found by experiment, that an eolipile, with its ſtem bent like the wire above mentioned, and ſuſpended on its center of gravity by a fine thread, will move in the ſame direction, whether it be throwing ſteam out at the orifice; or, after it is exhauſted, and cooling, it be drawing the air or water in.

WITH reſpect to the power of points, it has been obſerved by Mr. Villette of Liege, that a needle, concealed in a glaſs tube, which projected an inch beyond it, takes a ſtronger ſpark from a prime conductor than a man's finger; alſo that when the points of needles are covered with tallow, bees-wax, ſulphur, &c. they take peculiarly ſtrong ſparks. He adds, that, when ſulphur is uſed, and the ſparks are taken obliquely, they are ſometimes of a beautiful citron colour *.

Mr. LULLIN has made an additional obſervation to Monſieur du Fay's, concerning the different manner in which conducting and non-conducting ſubſtances are affected, when they are expoſed to the dew. He ſays, that if a plate of glaſs be extended upon ſilken threads, and expoſed to the open air all night, and a plate of metal, leſs than the glaſs, be laid upon it, in the morning, the metal will be dry, and likewiſe the glaſs, on both ſides, exactly under the metal; but that the edges of the glaſs, where the metal did not reach, will be wet on both ſides †.

I SHALL cloſe this ſection of miſcellaneous articles, and the whole hiſtory of electricity, with a ſuccinct account of ſome of the chief particulars in which the analogy between electricity and magnetiſm conſiſts; very nearly as it was drawn up, in an abridg-

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* Nollet's Letters, Vol. iii. p. 212.

† Ibid. p. 54.

ment of Mr. Æpinus, and communicated to me for this purpoſe by Dr. Price.

1. As a rod of iron held near a magnet will have ſeveral ſucceſſive poles; ſo will a glaſs tube touched by an excited tube have a ſucceſſion of poſitive and negative parts.

2. Bodies poſitively and negatively electrical, when in contact, will unite to one another; as will magnets, when they are laid with their oppoſite poles to one another.

3. Glass is a ſubſtance of a nature ſimilar to hardened ſteel. The poſitive and negative ſides of the former anſwer to the attracting and repelling ends of the latter, when magnetical.

4. As it is difficult to move the electric fluid in the pores of the former; ſo, likewiſe, it is difficult to move the magnetic fluid in the pores of the latter.

5. As there can be no condenſation of the electric fluid in the former, without a rarefaction; ſo, in the latter, if there be a condenſation, or poſitive magnetiſm in one end of a bar, there muſt be an evacuation, or negative magnetiſm in the other end.

6. Steel correſponds to electrics per ſe, and iron in ſome meaſure, to conductors of electricity.

7. Steel is leſs ſuſceptible of the magnetic virtue, but when it has acquired it, it retains it more ſtrongly than iron; juſt as electrics per ſe will not ſo eaſily receive the electric fluid, but, when it is forced into them, will retain it more ſtrongly than conductors.

8. Mr. Æpinus adds, and reckons it one of his diſcoveries, that an electrified body does not act on other bodies, except they are themſelves electrified; juſt as a magnet will not act on any other ſubſtances, except they are themſelves poſſeſſed of the magnetic virtue. So that an electrified body attracts and repels another body, only in conſequence of rendering it firſt of all electrical; as a magnet attracts iron, only in conſequence of, firſt of all, making it a magnet.

9. MR. CANTON has alſo found, that if the tourmalin be cut into ſeveral pieces, each piece will have a poſitive and negative ſide, juſt as the pieces of a broken magnet would have.

THUS far, ſays Dr. Price, there is an analogy, and, in ſome inſtances, a ſtriking one, between magnetiſm and electricity, upon the ſuppoſition that the cauſe of magnetiſm is a fluid. But there is no magnetic ſubſtance which anſwers perfectly to the conductors of electricity. There is no afflux or efflux of the magnetical fluid ever viſible. The equilibrium in a magnet cannot be inſtantaneouſly reſtored, by forming a communication between the oppoſite ends with iron, as it may in charged glaſs. Nor are there any ſubſtances poſitively or negatively magnetical only, as there are bodies which are poſitively or negatively electrical only.

PART II.

A

SERIES OF PROPOSITIONS.

COMPRISING ALL THE

GENERAL PROPERTIES OF ELECTRICITY.

AFTER tracing, at large, the progreſs of all the diſcoveries relating to electricity, and giving an hiſtorical account of them, in the order in which they were made; it will, probably, be no diſagreeable repetition, if I give, at the cloſe of it, a SERIES OF PROPOSITIONS, compriſing all the general properties of electricity, drawn up in as ſuccinct a manner as poſſible. And, notwithſtanding the large detail which has been made, it will be found, that a few propoſitions are ſufficient to compriſe almoſt all that we know of the ſubject.

THIS circumſtance may be regarded as a demonſtration of the real progreſs that has been made in this ſcience And as this progreſs advances, and the hiſtory enlarges, paradoxical as the aſſertion may ſeem, this part may be expected to contract itſelf in the ſame proportion. For the more we know of any ſcience, the greater number of particular propoſitions are we able to reſolve into general ones; and, conſequently, within narrower bounds ſhall we be able to reduce its principles.

I MIGHT have made this part of my work much ſhorter, even in the preſent ſtate of the ſcience, if I would have admitted into it any thing theoretical; but I have carefully avoided the principles of any theory, even the moſt probable, and the neareſt to being perfectly aſcertained, in this ſeries of propoſitions; in which I propoſe to comprehend only *known facts*; that my younger readers may carefully diſtinguiſh between *fact* and *theory*; things which are too often confounded.

I HAVE not, in this part of my work, deſcended to any *minutiæ*, in the deſcription of electrical appearances, becauſe they have been entered into before, and a repetition of them would have been tedious. At the ſame time, I think it will be found, upon examination, that I have not omitted to take notice of any diſcovery of importance. I have alſo introduced into it the definition of all the moſt neceſſary *technical terms*; that this part of the work might ſerve as a methodical introduction, to thoſe who are beginning the ſtudy of electricity, and deſire a general knowledge of the firſt elements of the ſcience, before they enter upon the detail of particulars, which will be beſt learned afterwards, from the hiſtory *.

* Leſt this part of the work ſhould not prove a ſufficient *introduction to the ſtudy of electricity*, I have ſince publiſhed a ſmall piece with this title. It contains a more familiar explication of the fundamental principles of electricity, mixing theory with facts, and illuſtrating, chiefly, thoſe experiments which are the moſt entertaining.

By *electricity*, in the following propoſitions, I would be underſtood to mean, only thoſe *effects* which will be called electrical; or elſe the *unknown cauſe* of thoſe effects, uſing the term, as we uſe the letters x and y in Algebra.

All known ſubſtances are diſtributed by electricians into two ſorts. Thoſe of one ſort are termed *electrics*, or *non-conductors*; and thoſe of the other *non-electrics*, or *conductors of electricity*.

Metals of all kinds, together with ſemi-metals and water, are conductors. So alſo is charcoal, and other ſubſtances of a ſimilar nature, as will be ſhewn at large in the laſt part of this work. All other ſubſtances, whether mineral, vegetable, or animal, are non-conductors. But many of theſe when they are made very hot, as glaſs, roſin, baked wood, and, perhaps, all the reſt on which the experiment can be made in this ſtate, are conductors of electricity.

All bodies, however, though in the ſame ſtate of heat and cold, are not equally perfect electrics, or perfect conductors. Vegetable and animal ſubſtances, for inſtance, in their natural ſtate, are ſeldom perfect electrics, on account of the moiſture that is contained in them. And, independent of moiſture, there is probably a gradation in all ſubſtances, from the moſt perfect conductors to the moſt perfect non-conductors of electricity.

It is the property of all kinds of electrics, that when they are rubbed by bodies differing from themſelves (in roughneſs or ſmoothneſs chiefly) to attract light bodies of all kinds which are preſented to them; to exhibit an appearance of light (which is very viſible in the dark) attended with a ſnapping noiſe, upon the approach of any conductor; and, if the noſtrils be preſented, they are affected with a ſmell like that of phoſphorus.

An electric ſubſtance exhibiting theſe appearances, is ſaid to be *excited*, and ſome of them, particularly the tourmalin, are excited by heating and cooling, as well as by rubbing.

It is neceſſary, however, to a conſiderable excitation of any electric, that the ſubſtance againſt which it is rubbed (uſually called the *rubber)* have a communication with the earth, or bodies abounding with electricity, by means of conductors; for if the rubber be *inſulated*, that is, if it be cut off from all communication with the earth by means of electrics, the friction has but little effect.

When inſulated bodies have been attracted by, and brought into contact with any excited electric, they begin to be repelled by it, and alſo to repel one another: nor will they be attracted again, till they have been in contact with ſome conductor communicating with the earth; but, after this, they will be attracted as at firſt.

If conductors be inſulated, electric powers may be *communicated* to them by the approach of excited electrics. They will then attract light bodies, and give ſparks, attended with a ſnapping noiſe, like the electrics themſelves. But there is this difference between excited and communicated electricity, that a conductor to which electricity has been communicated parts with its whole power at once, on the contact of a conductor communicating with the earth; whereas an excited electric, in the ſame circumſtances, loſes its electricity only partially; it being diſcharged only from the part which was actually touched by the conductor, or thoſe in the neighbourhood of it; ſo that the ſpark of electric fire is not ſo denſe, nor the exploſion made by parting with it ſo loud, from excited as from communicated electricity.

Electric ſubſtances brought into contact with excited electrics, will not deſtroy their electricity; whence it is that they are called non-conductors, becauſe they will not convey or *conduct* away whatever is the cauſe of electric appearances in bodies.

WHEN electricity is ſtrongly communicated to inſulated animal bodies, the pulſe is quickened, and perſpiration increaſed; and, if they receive, or part with their electricity on a ſudden, a painful ſenſation is felt at the place of communication.

THE growth of vegetables is quickened by electricity.

No electric can be excited without producing electric appearances in the body with which it is excited, provided that body be inſulated. For this inſulated rubber will attract light bodies, give ſparks, and make a ſnapping noiſe upon the approach of a conductor, as well as the excited electric.

IF an inſulated conductor be pointed, or if a pointed conductor communicating with the earth be held pretty near it, little or no electric appearance will be exhibited; only a light will appear at each of the points, during the act of excitation, and a current of air will be ſenſible from off them both.

THESE two electricities, viz. that of the electric itſelf, and that of the rubber, though ſimilar to, are the reverſe of one another. A body attracted by the one will be repelled by the other, and they will attract, and in all reſpects act upon one another more ſenſibly than upon other bodies; ſo that two pieces of glaſs or ſilk, poſſeſſed of contrary electricities, will cohere firmly together, and require a conſiderable force to ſeparate them.

THESE two electricities, having been firſt diſcovered by producing one of them from glaſs, and the other from amber, ſealing-wax, ſulphur, roſin, &c. firſt obtained the names of *vitreous* and *reſinous electricity*; and it being afterwards imagined that one of them was a redundancy, and the other a deficiency of a ſuppoſed electric fluid, the former (viz. that which is produced from the friction of ſmooth glaſs tubes or globes by the human hand, or a common leathern rubber) obtained the name of *poſitive*; and the latter (viz. that which is produced from the friction of ſticks or globes of ſulphur, &c. or collected from the rubber of a glaſs globe above mentioned) that of *negative* electricity: and theſe terms are now principally in uſe.

IF a conductor, not inſulated, be brought within the *atmoſphere*, that is, the ſphere of action, of any electrified body, it acquires the electricity oppoſite to that of the electrified body; and the nearer it is brought, the ſtronger oppoſite electricity doth it acquire, till the one receive a ſpark from the other, and then the electricity of both will be diſcharged.

THE electric ſubſtance which ſeparates the two conductors, poſſeſſing theſe two oppoſite kinds of electricity, is ſaid to be *charged*. Plates of glaſs are the moſt convenient for this purpoſe, and the thinner the plate, the greater charge it is capable of holding. The conductors contiguous to each ſide of the glaſs are called their *coating*.

AGREEABLE to the above-mentioned general principle, it is neceſſary, that one ſide of the charged glaſs have a communication with the rubber, while the other receives the electricity from the conductor, or with the conductor, while the other receives from the rubber.

IT follows alſo, that the two ſides of the plate thus charged are always poſſeſſed of the two oppoſite electricities; that ſide which communicates with the excited electric having the electricity of the electric, and that which communicates with the rubber, that of the rubber.

THERE is, conſequently, a every eager attraction between theſe two electricities with which the different ſides of the plate are charged; and, when a proper communication is made by means of conductors, a flaſh of electric light, attended with a report (which is greater or leſs in proportion to the quantity of electricity communicated to them, and the goodneſs of the conductors) is perceived between them, and the electricity of both ſides is thereby diſcharged.

The ſubſtance of the glaſs itſelf, in or upon which theſe electricities exiſt, is impervious to electricity, and does not permit them to unite; but if they be very ſtrong, and the plate of glaſs very thin, they will force a paſſage through the glaſs. This, however, always breaks the glaſs, and renders it incapable of another charge.

The flaſh of light, together with the exploſion between the two oppoſite ſides of a charged electric, is generally called the *electric ſhock*, on account of the diſagreeable ſenſation it gives any animal, whoſe body is made uſe of to form the communication between them.

This electric ſhock is always found to perform the circuit from one ſide of the charged glaſs to the other by the ſhorteſt paſſage, through the beſt conductors. Common communicated electricity alſo obſerves the ſame rule, in its tranſmiſſion from one body to another.

It has not been found that the electric ſhock, takes up the leaſt ſenſible ſpace of time in being tranſmitted to the greateſt diſtances.

The electric ſhock, as alſo the common electric ſpark, diſplaces the air through which it paſſes; and if its paſſage from conductor to conductor be interrupted by non-conductors of a moderate thickneſs, it will rend and tear them in its paſſage; and in ſuch a manner as to exhibit the appearance of a ſudden expanſion of the air about the center of the ſhock.

If an electric ſhock, or ſtrong ſpark, be made to paſs through, or over the belly of a muſcle, it forces it to contract as in a convulſion.

If a ſtrong ſhock be ſent through a ſmall animal body, it will often deprive it inſtantly of life.

When the electric ſhock is very ſtrong, it will give polarity to magnetic needles, and ſometimes it reverſes their poles.

Great ſhocks, by which animals are killed, are ſaid to haſten putrefaction.

Electricity and lightning are, in all reſpects, the ſame thing. Every effect of lightning may be imitated by electricity, and every experiment in electricity may be made with lightning, brought down from the clouds, by means of inſulated pointed rods of metal.

PART III.

THEORIES OF ELECTRICITY.

SECTION I.

OF PHILOSOPHICAL THEORIES IN GENERAL, AND THE THEORIES OF ELECTRICITY PRECEDING THAT OF DR. FRANKLIN.

ONE of the moſt intimate of all aſſociations in the human mind is that of *cauſe* and *effect*. They ſuggeſt one another with the utmoſt readineſs upon all occaſions; ſo that it is almoſt impoſſible to contemplate the one, without having ſome idea of, or forming ſome conjecture about the other. In viewing the works of nature, we neceſſarily become firſt acquainted with appearances or effects. We naturally attend to the circumſtances in which ſuch appearances always ariſe, and cannot help conſidering them as the cauſes of thoſe appearances. Then, conſidering

ing theſe circumſtances themſelves as new appearances, we are deſirous of tracing out other circumſtances that gave birth to them. Thus, conſtantly aſcending in this chain of cauſes and effects, we are led, at laſt, to the firſt cauſe of all: and then we conſider all ſecondary and inferior cauſes, as nothing more than the various methods in which the ſupreme cauſe acts, in order to bring about his great deſigns.

In all ſcience, we firſt aſcend from particular to general. For nature exhibits nothing but particulars; and all general propoſitions, as well as general terms, are artificial things, being contrived for the eaſe of our conception and memory; in order to comprehend things clearly, and to compriſe as much knowledge as poſſible in the ſmalleſt compaſs. It is no wonder then that we take pleaſure in this proceſs. Beſides, we actually ſee in nature a vaſt variety of effects proceeding from the ſame general principles, operating in different circumſtances; ſo that judging from appearances, that nature is every where uniform with itſelf, we are led, by analogy, to expect the ſame in all caſes, and think it an argument in favour of any ſyſtem, if it exhibits a variety of effects ſpringing from a few cauſes. For ſuch variety in effects, and ſuch ſimplicity in cauſes, we generally ſee in nature.

Having diſcovered the cauſe of any appearance, it is the buſineſs of philoſophy to trace it in all its effects, and to predict other ſimilar appearances from ſimilar previous ſituations of things. By this means, the true philoſopher, knowing what will be the reſult of putting every thing, which the preſent ſyſtem exhibits, into every variety of circumſtances, is maſter of all the powers of nature, and can apply them to all the uſeful purpoſes of life. Thus does *knowledge*, as Lord Bacon obſerves, become *power*; and thus is the philoſopher capable of providing, in a more effectual manner, both for his own happineſs and for that of others; and thereby of approving himſelf a good citizen, and an uſeful member of ſociety.

It is obvious, from this general view of the buſineſs of philoſophy, that, in order to trace thoſe circumſtances in which any appearance in nature is certainly and invariably produced, it is chiefly uſeful to obſerve what there is *in common* in the circumſtances attending ſimilar appearances: for on thoſe common circumſtances, all that is common in the appearances muſt depend. And the eaſieſt poſſible method, by which we can trace out the connection of cauſes and effects in nature, is to begin with comparing thoſe appearances which are moſt ſimilar, where the difference conſiſts in a ſingle circumſtance; the whole effect of which, in different appearances, is thereby perfectly known. And when we have, by this means, noted the whole effect of all the ſeparate circumſtances and ſituations of things, we can eaſily judge of their effect in all poſſible combinations.

Hence *analogy* is our beſt guide in all philoſophical inveſtigations; and all diſcoveries, which were not made by mere accident, have been made by the help of it. We obſerve a complex appearance, attended with a proportionable variety of circumſtances and ſituations. We alſo ſee another appearance, in ſome reſpects ſimilar, in others diſſimilar; the circumſtances being, likewiſe, ſimilar and diſſimilar in the ſame proportion; or we purpoſely vary the circumſtances of the former appearance, and obſerve what difference it occaſions. But, unleſs there be a very great analogy, or ſimilarity, between them, ſo that the influence of a ſingle circumſtance, or of a few circumſtances, can be traced ſeparately, no probable judgment can be formed of their real operation.

But in all this proceſs, a man who acts from deſign, and not abſolutely at random, would never think of trying the influence of any circumſtance in an appearance, unleſs, from ſome other analogies in nature, more or leſs perfect, he had formed ſome idea what its influence would probably be; at leaſt, he muſt, from analogies in nature previouſly obſerved, have formed an idea of ſeveral poſſible conſequences, and try which of them will really follow. That is, in other words, every experiment, in which there is any deſign, is made to aſcertain ſome *hypotheſis*. For an hypotheſis is nothing more than a preconceived idea of an event, as ſuppoſed to ariſe from certain circumſtances, which muſt have been imagined to have produced the ſame, or a ſimilar effect, upon other occaſions. An hypotheſis abſolutely verified ceaſes to be termed ſuch, and is conſidered as a fact; though, when it has long been in an hypothetical ſtate, it may continue to be called, occaſionally, by the ſame name.

THE only danger in the uſe of hypotheſes ariſes from making this tranſition too ſoon. And when an hypotheſis is no longer conſidered as a mere probable ſuppoſition, but a real fact; a philoſopher not only acquieſces in it, and thereby miſtakes the cauſe of one particular appearance; but, by its analogies, he miſtakes the cauſe of other appearances too, and is led into a whole ſyſtem of error. A philoſopher who has been long attached to a favourite hypotheſis, and eſpecially if he have diſtinguiſhed himſelf by his ingenuity in diſcovering or purſuing it, will not, ſometimes, be convinced of its falſity by the plaineſt evidence of fact. Thus both himſelf, and all his followers, are put upon falſe purſuits, and ſeem determinedto warp the whole courſe of nature, to ſuit their manner of conceiving of its operations.

BUT, provided philoſophers can be upon their guard againſt this ſpecies of vanity (which muſt be owned to be very tempting) and againſt the obſtinacy which is the conſequence of it; hypotheſes, and even a great variety of them, are certainly very promiſing circumſtances to philoſophical diſcoveries. Hypotheſes, while they are conſidered merely as ſuch, lead perſons to try a variety of experiments, in order to aſcertain them. In theſe experiments, new facts generally ariſe. Theſe new facts ſerve to correct the hypotheſis which gave occaſion to them. The theory, thus corrected, ſerves to diſcover more new facts, which, as before, bring the theory ſtill nearer to the truth. In this progreſſive ſtate, or method of approximation, things continue; till, by degrees, we may hope that we ſhall have diſcovered all the facts, and have formed a perfect theory of them. By this perfect theory, I mean a ſyſtem of propoſitions, accurately defining all the circumſtances of every appearance, the ſeparate effect of each circumſtance, and the manner of its operation.

I HAVE dwelt ſo long upon this ſubject, becauſe I apprehend, that electricians have generally been too much attached to their ſeveral theories, ſo as to have retarded the progreſs of real diſcoveries. Indeed, no other part of the whole compaſs of philoſophy affords ſo fine a ſcene for ingenious ſpeculation. Here the imagination may have full play, in conceiving of the manner in which an inviſible agent produces an almoſt infinite variety of viſible effects. As the agent is inviſible, every philoſopher is at liberty to make it whatever he pleaſes, and aſcribe to it ſuch properties and powers as are moſt convenient for his purpoſe. And, indeed, if he can frame his theory ſo as really to ſuit all the facts, it has all the evidence of truth that the nature of things can admit.

WITH the firſt electricians, electrical attraction was performed by means of *unctuous effluvia* emitted by the excited electric. Theſe were ſuppoſed to faſten upon all bodies in their way, and to carry back with them all that were not too heavy. For, in that age of philoſophy, all effluvia were ſuppoſed to return to the bodies from which they were emitted; ſince no perſon could, otherwiſe, account for the ſubſtance not being ſenſibly

waſted

waſted by the conſtant emiſſion. When theſe light bodies, on which the unctuous effluvia had faſtened, were arrived at the excited electric, a freſh emiſſion of the effluvia was ſuppoſed to carry them back again. But this effect of the effluvia was not thought of, till electrical repulſion had been ſufficiently obſerved.

WHEN the Newtonian philoſophy had made ſome progreſs, and the extreme ſubtilty of light, and other effluvia of bodies, was demonſtrated; ſo that philoſophers were under no apprehenſion of bodies being waſted by continual emiſſion, the doctrine of *the return of the effluvia* was univerſally given up as no longer neceſſary, and they were obliged to acquieſce in the unknown principles of attraction and repulſion, as ſuppoſed to be properties of certain bodies, communicated to them by the Divine Being, the mechanical cauſe of which they ſcarce attempted to explain.

WHEN Mr. Du Fay diſcovered the two oppoſite ſpecies of electricity, which he termed the *vitreous* and *reſinous* electricity, he neceſſarily formed the idea of *two diſtinct electric fluids*, repulſive with reſpect to themſelves, and attractive of one another. But he had no idea of both ſpecies being actually concerned in every electrical operation, and that glaſs or roſin alone always produced them both. This theory, therefore, was as ſimple in its application as the other.

WHILE nothing more was known of electricity but attraction and repulſion, this general theory was ſufficient. The general attraction of all bodies to all bodies was called (and by ſome abſurdly enough ſuppoſed to be accounted for by) gravitation, and many ſuperficial philoſophers thought they had given a very good account of electricity, coheſion, and magnetiſm, by calling them particular ſpecies of attraction peculiar to certain bodies.

BUT when electricity began to ſhow itſelf in a greater variety of appearances, and to make itſelf ſenſible to the ſmell, the ſight, the touch, and the hearing: when bodies were not only attracted and repelled, but made to emit ſtrong ſparks of fire, attended with a conſiderable noiſe, a painful ſenſation, and a ſtrong phoſphoreal ſmell; electricians were obliged to make their ſyſtems more complex, in proportion as the facts were ſo. It was then generally ſuppoſed, that the matter of the electric fluid was the ſame with the chymical principle of fire; though ſome thought it was a fluid *ſui generis*, which very much reſembled that of fire; and others, with Mr. Boulanger at their head, thought that the electric fluid was nothing more than the finer parts of the atmoſphere, which crouded upon the ſurfaces of electric bodies, when the groſſer parts had been driven away by the friction of the rubber.

THE great difficulty common to all theſe theories was to aſcertain the direction of the electric matter. It is no wonder that, when electrical appearances were firſt obſerved, all electric powers were ſuppoſed to reſide in, and therefore to proceed from the excited electric. Conſequently, the electric ſpark was firſt imagined to be darted from the electrified body towards any conductor that was preſented to it. It was never imagined there could be any difference in this reſpect whether it was amber, glaſs, ſealing-wax, or any thing elſe that was excited. Nothing was thought to be more evident to the ſenſes than this progreſs of the electric matter: what then muſt have been the aſtoniſhment of all electricians, when they firſt obſerved electric appearances at an inſulated rubber; at the ſame time that it was demonſtrated, that the action of the rubber did not *produce*, but only *collect* the electric fluid.

IN this caſe, the current could not have been ſuppoſed to flow both from the conductor and the rubber; and yet the firſt appearances were the ſame. To provide a ſupply of the electric matter, they were obliged to ſuppoſe that, notwithſtanding appearances were nearly the ſame, the electric fluid was really received by the electrified body in the one caſe, and emitted by it in the other. But now, being obliged to give up the argument for the manner of its progreſs from the evidence of ſight, they were at a loſs whether, in the uſual method of electrifying by excited glaſs, the fluid proceeded from the rubber to the conductor, or from the conductor to the rubber; and nothing was found to obviate theſe difficulties, till an excellent theory of poſitive and negative electricity was ſuggeſted by Dr. Watſon, and digeſted and illuſtrated by Dr. Franklin.

It was ſoon found, that the electricity, at the rubber was the reverſe of that at the conductor, and in all reſpects the ſame with that which had before been produced by the friction of ſealing-wax, ſulphur, roſin, &c. Seeing, therefore, that both the elecricities, as they had heretofore been called, were produced at the ſame time, by one and the ſame electric, and by the ſame friction, all electricians, and among the reſt Mr. Du Fay himſelf, concluded, that they were both modifications of one and the ſame fluid; and the old doctrine of the different electricities was univerſally diſcarded.

The accidental diſcovery of the Leyden phial moſt clearly demonſtrated the imperfection of all the theories preceding that of poſitive and negative electricity, by exhibiting an aſtoniſhing appearance, which no electricians, with the help of any theory, could have foreſeen, and of which they could have formed no idea, *a priori*.

Upon this great event, new theories of electricity multiplied apace, ſo that it would be to no purpoſe to enumerate them all. Indeed many of them were no more than the beings of a day. For no ſooner were they ſtarted, but the authors themſelves, upon the appearance of ſome new fact, ſaw reaſon to new model, or entirely reject them. I ſhall, therefore, content myſelf with giving the outlines of ſome of the principal theories of electricity, which have their adherents at preſent, without conſidering whether they took their riſe before or after this diſcovery.

WITH ſome, and particularly Mr. Wilſon, the chief agent in all electrical operations is Sir Iſaac Newton's ether; which is more or leſs denſe in all bodies, in proportion to the ſmallneſs of their pores, except that it is much denſer in ſulphureous and unctuous bodies *. To this ether are aſcribed the principal phenomena of attraction and repulſion, whereas the light, the ſmell, and other ſenſible qualities of the electric fluid are referred to the groſſer particles of bodies, driven from them by the forcible action of this ether. Many phenomena in electricity are alſo attempted to be explained by means of a ſubtile medium, at the ſurface of all bodies, which is the cauſe of the refraction and reflection of the rays of light, and alſo reſiſt the entrance and exit of this ether †. This medium, he ſays, extends to a ſmall diſtance from the body, and is of the ſame nature with what is called the electric fluid. On the ſurface of conductors this medium is rare, and eaſily admits the paſſage of the electric fluid; whereas on the ſurface of electrics it is denſe, and reſiſts it. This medium is rarefied by heat, which converts non-conductors into conductors ‡. On this theory I ſhall make no particular remarks, becauſe I cannot ſay that I clearly comprehend it.

BUT the far greater number of philoſophers ſuppoſe, and with the greateſt probability, that there is a fluid *ſui generis* principally concerned in the buſineſs of electricity. They ſeem, however, though perhaps without reaſon, entirely to overlook Sir

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* Wilſon's Diſſertation, p. 5.

† Headley and Wilſon, p. 55.

‡ Ibid. p. 78.

Iſaac Newton's ether; or if they do not ſuppoſe it to be wholly unconcerned, they allow it only a ſecondary and ſubordinate part to act in this drama. And among thoſe who ſuppoſe a fluid *ſui generis*, there is a great diverſity of opinions about the mode of its exiſtence, and the manner of its operation.

THE ingenious Abbé Nollet, whoſe theory has been more the ſubject of debate than all the other theories before Dr. Franklin's, ſuppoſes that, in all electrical operations, the fluid is thrown into two oppoſite motions; that the *affluence* of this matter drives all light bodies before it, by impulſe, upon the electrified body, and its *effluence* carries them back again. But he ſeems very much embarraſſed in accounting for facts where both theſe currents muſt be conſidered, at the ſame time that he is obliged to find expedients to prevent their impeding the effects of each other. To obviate this great difficulty, he ſuppoſes, that every excited electric, and likewiſe every body to which electricity is communicated, has two orders of pores, one for the emiſſion of the effluvia, and the other for the reception of them. A man of leſs ingenuity than the Abbé could not have maintained himſelf in ſuch a theory as this; but, with his fund of invention, he was never at a loſs for reſources upon all emergencies, and in his laſt publication appears to be as zealous for this ſtrange hypotheſis as at the firſt.

HE more than once requeſted a deputation of the members of the Academy of Sciences, to be witneſſes of ſome experiments, in which, he thought, there was a viſible effluence of the electrical effluvia from the conductor, both to the globe at one of its extremities, and to any non-electric preſented to it at the other; and their teſtimony was ſigned and regiſtered in proper form *.

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* Leçons de Phyſique, p. 368. 395.

But it does not ſeem to the honour of Mr. Nollet, or thoſe gentlemen of the Academy, to be ſo very poſitive in a matter which does not admit of the evidence of ſenſe.

THE Abbé's confidence upon this ſubject is very remarkable. Theſe effects, ſays he, well conſidered, and reviewed a thouſand times, in the courſe of thirty years, in which I have applied to electricity, make me ſay with confidence, that thoſe pencils of rays are currents of electric matter, which fly from the conductor towards the excited globe. This is ſo evident, that I would freely appeal to the ocular teſtimony of any unprejudiced perſon, who ſhould ſee the experiments which I have recited. But, ſays he, the fact in queſtion is contrary to a ſyſtem of electricity, which ſome perſons perſiſt in maintaining. They have the aſſurance to tell me, that the matter of the luminous pencil, in my experiment, moves in a direction quite oppoſite to that which I ſuppoſe; that it proceeds from the excited globe, and is from thence thrown upon any non-electric within its reach *. In another place, he ſays, that the principle of ſimultaneous effluences and affluences is by no means a *ſyſtem*, but a *fact* well proved †.

THE Abbé Nollet propoſes an hypotheſis to explain the difference between common electricity and the electric ſhock. All the effects of common electricity, he ſays, plainly ſhow, that the electric matter is animated with a progreſſive motion, which really carries it forwards; whereas the remarkable caſe of the electric ſhock appears to be an inſtantaneous percuſſion, which the contiguous parts of the ſame matter communicate to one another, without being diſplaced. Sound and wind, he ſays, are motions of the air; but would a philoſopher, be permitted to take the one for the other, in meaſuring their velocity or extent. But this compariſon is by no means juſt ‡.

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* Leçons de Phyſique, p. 363.

† Lettres ſur l'Electricité, p. 98.

‡ Leçons de Phyſique, p 293.

It muſt be acknowledged, that far the greater part of the Abbé Nollet's arguments in favour of his doctrine of effluences and affluences are very unſatisfactory, and that his method of accounting for electrical attraction and repulſion, with other phenomena in electricity, by means of it, is more ingenious than ſolid. It is a great pity that this truly excellent philoſopher had not ſpent more time in diverſifying facts, and leſs in refining upon theory. But it is in ſome meaſure the natural fault of a diſpoſition to philoſophize.

Mr. Du Tour improves upon this hypotheſis of the Abbé Nollet, by ſuppoſing that there is a difference between the affluent and effluent current; and that the particles of the fluid are thrown into vibrations of different qualities, which makes one of theſe currents more copious than the other, according as ſulphur or glaſs is uſed. Difficult as it is to form any idea of this hypotheſis, the author appears very much attached to it, and has no doubt of its accounting for all electrical appearances.

SECTION II.

THE THEORY OF POSITIVE AND NEGATIVE ELECTRICITY.

THE Engliſh philoſophers, and perhaps the greater part of foreigners too, have now generally adopted the theory of *poſitive* and *negative* electricity. As this theory has been extended to almoſt all the phenomena, and is the moſt probable of any that have been hitherto propoſed to the world, I ſhall give a pretty full account of it, and ſhow how it agrees with all the propoſitions of the laſt part, to which it has hitherto been applied.

THIS theory generally goes by the name of Dr. Franklin, and there is no doubt of his right to it; but juſtice requires that I diſtinctly mention the equal, and, perhaps, prior claim of Dr. Watſon, to whom I have before ſaid it had occurred. Dr. Watſon ſhowed a ſeries of experiments to confirm the doctrine of *plus* and *minus* electricity to Martin Folkes, Eſq. then preſident, and to a great number of fellows of the Royal Society, ſo early as the beginning of the year 1747, before it was known in England that Dr. Franklin had diſcovered the ſame thing in America. See the Philoſophical Tranſactions, vol. xliv. p. 739; and vol. xlv. p. 93—101. Dr. Franklin's paper, containing the ſame diſcovery, was dated at Philadelphia, June the 1ſt, 1747.

ACCORD-

ACCORDING to this theory, all the operations of electricity depend upon one fluid *ſui generis*, extremely ſubtile and elaſtic, diſperſed through the pores of all bodies; by which the particles of it are as ſtrongly attracted, as they are repelled by one another.

WHEN the equilibrium of this fluid in any body is not diſturbed; that is, when there is in any body neither more nor leſs of it than its natural ſhare, or than that quantity which it is capable of retaining by its own attraction, it does not diſcover itſelf to our ſenſes by any effect. The action of the rubber upon an electric diſturbs this equilibrium, occaſioning a deficiency of the fluid in one place, and a redundancy of it in another.

THIS equilibrium being forcibly diſturbed, the mutual repulſion of the particles of the fluid is neceſſarily exerted to reſtore it. If two bodies be both of them overcharged, the electric atmoſpheres (to adopt the ideas of all the patrons of this hypotheſis before Mr. Æpinus) repel each other, and both the bodies recede from one another to places where the fluid is leſs denſe. For, as there is ſuppoſed to be a mutual attraction between all bodies and the electric fluid, electrified bodies go along with their atmoſpheres. If both the bodies be exhauſted of their natural ſhare of this fluid, they are both attracted by the denſer fluid, exiſting either in the atmoſphere contiguous to them, or in other neighbouring bodies; which occaſions them ſtill to recede from one another, as much as when they were overcharged.

SOME of the patrons of the hypotheſis of poſitive and negative electricity conceive otherwiſe of the immediate cauſe of this repulſion. They ſay that, as the denſer electric fluid, ſurrounding two bodies negatively electrified, acts equally on all ſides of thoſe bodies, it cannot occaſion their repulſion. Is not the repulſion, ſay they, owing rather to an accumulation of the electric fluid on the ſurfaces of the two bodies; which accumulation is produced by the attraction of the bodies, and the difficulty the fluid finds in entering them? This difficulty in entering is ſuppoſed to be owing, chiefly, to the *air* on the ſurface of bodies, which is probably a little condenſed there; as may appear from Mr. Canton's experiment above mentioned on the double barometer.

Lastly, if one of the bodies have an overplus of the fluid, and the other a deficiency of it, the equilibrium is reſtored with great violence, and all electrical appearances between them are more ſtriking.

The influence of *points* in drawing or throwing off the electric fluid has not been quite ſatisfactorily accounted for upon any hypotheſis, but it is as agreeable to this as any other. As it is evident that every electric atmoſphere meets with ſome reſiſtance, both in entering and quitting any body, whatever be the cauſe of that reſiſtance, it is natural to ſuppoſe, that it muſt be leaſt at the points of bodies where there are fewer particles of the body (on which the reſiſtance depends) oppoſed to its paſſage, than at the flat parts of the ſurface, where the reſiſting power of a greater number of particles is united.

The *light* which is viſible in electrical appearances is generally ſuppoſed to be part of the compoſition of the electric fluid, which appears when it is properly agitated. But this ſuppoſition concerning electric light is not neceſſary to the general hypotheſis. It may be ſuppoſed, upon this as well as Mr. Wilſon's theory, that the light, and the phoſphoreal ſmell, in electrical experiments ariſe from particles of matter much groſſer than the proper electric fluid, but which may be driven from bodies by its powerful action.

The *ſound* of an electrical exploſion is certainly produced by the air being diſplaced by the electrical fluid, and then ſuddenly collapſing;

collapſing, ſo as to occaſion a vibration, which diffuſes itſelf every way from the place where the exploſion was made. For in ſuch vibrations ſound is known to conſiſt.

BUT the chief excellence of this theory of poſitive and negative electricity, and that which gave it the greateſt reputation, is the eaſy explication which it ſuggeſts of all the phenomena of the Leyden phial. This fluid is ſuppoſed to move with the greateſt eaſe in bodies which are conductors, but with extreme difficulty in electrics *per ſe*; inſomuch that glaſs is abſolutely impermeable to it. It is moreover ſuppoſed, that all electrics (and particularly glaſs) on account of the ſmallneſs of their pores, do at all times contain an exceedingly great, and always an equal quantity of this fluid; ſo that no more can be thrown into one part of any electric ſubſtance, except the ſame quantity go out at another, and the gain be exactly equal to the loſs. Theſe things being previouſly ſuppoſed, the phenomena of charging and diſcharging a plate of glaſs admit of an eaſy ſolution.

IN the uſual manner of electrifying, by a ſmooth glaſs globe, all the electric matter is ſupplied by the rubber from all the bodies which communicate with it. If it be made to communicate with nothing but one of the coatings of a plate of glaſs, while the conductor communicates with the other, that ſide of the glaſs which communicates with the rubber muſt neceſſarily be exhauſted, in order to ſupply the conductor, which muſt convey the whole of it to the ſide with which it communicates. By this operation, therefore, the electric fluid becomes almoſt entirely exhauſted on one ſide of the plate, while it is as much accumulated on the other; and the diſcharge is made by the electric fluid ruſhing, as ſoon as an opportunity is given it, by means of proper conductors, from the ſide which was overloaded to that which was exhauſted.

IT is not neceſſary, however, to this theory, that the very ſame individual particles of electric matter which were thrown upon one ſide of the plate, ſhould make the whole circuit of the intervening conductors, eſpecially in very great diſtances, ſo as actually to arrive at the exhauſted ſide. It may be ſufficient to ſuppoſe, as was obſerved before, that the additional quantity of fluid diſplaces and occupies the ſpace of an equal portion of the natural quantity of fluid belonging to thoſe conductors in the circuit, which lay contiguous to the charged ſide of the glaſs. This diſplaced fluid may drive forwards an equal quantity of the ſame matter in the next conductors; and thus the progreſs may continue, till the exhauſted ſide of the glaſs is ſupplied by the fluid naturally exiſting in the conductors contiguous to it. In this caſe the motion of the electric fluid in an exploſion will rather reſemble the vibration of the air in ſounds, than a current of it in winds.

It will eaſily be acknowledged, that while the ſubſtance of the glaſs is ſuppoſed to contain as much as it can poſſibly hold of the electric fluid, no part of it can be forced into one of the ſides, without obliging an equal quantity to quit the other ſide; but it may be thought a difficulty upon this hypotheſis, that one of the ſides of a glaſs plate cannot be *exhauſted*, without the other receiving more than its natural ſhare, particularly as the particles of this fluid are ſuppoſed to be repulſive of one another. But it muſt be conſidered, that the attraction of the glaſs is ſufficient to retain even the large quantity of the electric fluid which is natural to it, againſt all attempts to withdraw it, unleſs that eager attraction can be ſatisfied by the admiſſion of an equal quantity from ſome other quarter. When this opportunity of a ſupply is given, by connecting one of the coatings with the rubber, and the other with the conductor, the two attempts to introduce more of the fluid into one of the ſides, and to take from it on the other, are made, in a manner, at the ſame inſtant. The action of the rubber tends to diſturb the equilibrium of the fluid in the glaſs, and no ſooner has a ſpark quitted one of the ſides, to go to the rubber, but it is ſupplied by the conductor on the other; and the difficulty with which theſe additional particles move in the ſubſtance of the glaſs effectually prevents it reaching the oppoſite exhauſted ſide; near as the two ſides are to one another, and eager as is the endeavour of the fluid to go whither it is ſo ſtrongly attracted.

It is not ſaid, however, but that either ſide of the glaſs may give or receive *a ſmall quantity* of the electric fluid, without altering the quantity on the oppoſite ſide. It is only a very conſiderable part of the charge that is meant, when one ſide is ſaid to be filled, while the other is exhauſted.

It is a little remarkable, that the electric fluid, in this, and in every other hypotheſis, ſhould ſo much reſemble the ether of Sir Iſaac Newton in ſome reſpects, and yet differ from it ſo eſſentially in others. The electric fluid is ſuppoſed to be, like ether, extremely ſubtile and elaſtic, that is, repulſive of itſelf; but, inſtead of being, like the ether, repelled by all other matter, it is ſtrongly attracted by it; ſo that, far from being, like the ether, rarer in the ſmall than in the large pores of bodies, rarer within the bodies than at their ſurfaces, and rarer at their ſurfaces than at any diſtance from them; it muſt be denſer in ſmall than in large pores, denſer within the ſubſtance of bodies than at their ſurfaces, and denſer at their ſurfaces than at a diſtance from them. But no other property can account for the extraordinary quantity of this fluid contained within the ſubſtance of electrics *per ſe*, or for the common atmoſpheres of all excited and electrified bodies.

To account for the attraction of light bodies, and other electrical appearances, in air of the ſame denſity with the common atmoſphere, when glaſs (which is ſuppoſed to be impermeable to electricity) is interpoſed; it is conceived, that the addition or ſubſtraction of the electric fluid, by the action of the excited electric, on one ſide of the glaſs, occaſions (as in the experiment of the Leyden phial) a ſubſtraction or addition of the fluid on the oppoſite ſide. The ſtate of the fluid, therefore, on the oppoſite ſide being altered, all light bodies within the ſphere of its action muſt be affected, in the very ſame manner as if the effluvia of the excited electric had actually penetrated the glaſs, according to the opinion of all electricians before Dr. Franklin.

The manner in which *clouds* acquire their poſitive or negative electricity is not determined, according to this, or any other theory, with ſufficient certainty. Mr. Canton's conjecture is, that the air reſembles the tourmalin, and, conſequently, acquires its electricity by heating or cooling; but whether it gains or loſes, the electric fluid in either ſtate muſt be determined by experiment. Signior Beccaria's theory of the electricity of the clouds has been related at large.

This hypotheſis of poſitive and negative electricity has been adopted, and, in ſome meaſure, rendered more ſyſtematical by Mr. Æpinus, in his elaborate treatiſe entitled, *Tentamen Theoriæ Electricitatis et Magnetiſmi*.

He has extended the property of impermeability to air and all electrics as well as glaſs, and defined it in a better manner; ſuppoſing impermeability to conſiſt in the great difficulty with which electric ſubſtances admit the electric fluid into their pores, and the ſlowneſs with which it moves in them. Moreover, in conſequence of this impermeability of air to the electric fluid, he denies the reality of electric atmoſpheres, and thinks, as was obſerved before, that Dr. Franklin's theory will do much better without them.

He thinks that all the particles of matter muſt repel one another; for that, otherwiſe (ſince all ſubſtances have in them a certain quantity of the electric fluid, the particles of which repel one another, and are attracted by all other matter) it could not happen, that bodies in their natural ſtate, with reſpect to electricity, ſhould neither attract nor repel one another.

He that reads the firſt chapter, as well as many other parts of his elaborate treatiſe above mentioned, may ſave himſelf a good deal of time and trouble by conſidering, that the reſult of many of his reaſonings and mathematical calculations cannot be depended upon; becauſe he ſuppoſes the repulſion or elaſticity of the electric fluid to be in proportion to its condenſation; which is not true, unleſs the particles repel one another in the ſimple reciprocal ratio of their diſtances, as Sir Iſaac Newton has demonſtrated, in the ſecond book of his Principia.

Mr. Wilcke, as well as Mr. Æpinus, adopts all the general principles of Dr. Franklin's theory of poſitive and negative electricity, but thinks that no experiments which have hitherto been made ſhow which of the electricities is poſitive and which negative. Suppoſing, however, what is called poſitive to be really ſo, and that ſmooth glaſs, for inſtance, rubbed upon ſulphur attracts the electric fluid from it, he would account for it upon the ſame principles whereby water ſtands in drops on rough ſurfaces, but is diffuſed on ſmooth ones. The electric fluid, he would ſuppoſe, is more ſtrongly attracted by the ſmooth ſurface of the glaſs, and therefore diffuſes itſelf over it, while it retreats from electrics of rougher ſurfaces *. But this explanation, I imagine, will give little ſatisfaction to ſceptical electricians.

Mr. Wilcke acknowledges there is great difficulty in accounting for the repulſive power of bodies electrified negatively, and thinks that it obliges us to ſuppoſe the mutual repulſion of all homogeneous matter. Mr. Waitz, he ſays, was of the ſame opinion.

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* Wilcke, p. 65.

opinion. According to him, therefore, bodies which have too great a proportion either of the electric fluid, or of their proper conſtituent matter, muſt avoid one another. In the former caſe, by the repulſion of the electric fluid, in the latter, by the repulſion of the conſtituent parts of the bodies. Mr. Wilcke obſerves upon this ſubject, that the attraction of light bodies to negative electrics cannot be owing to the repulſive power of the electric fluid in the neighbouring air, driving them, or the electric matter in them to the place where there is a want of it; becauſe the velocity ought to decreaſe as it recedes from the impulſive power: whereas it is accelerated, as if it were attracted by the negative electric*.

But to this it may be replied, that a ſucceſſion of impulſes, though every ſubſequent one ſhould be weaker than the preceding, will produce an accelerated motion. Beſides, the nearer the light body is to the negative electric, the nearer it is to the point where the equilibrium of the fluid is moſt deſtroyed; or the leſs force there is on the ſide of the electric to balance the force that drives the light body towards it, and therefore the impulſes themſelves muſt increaſe.

Mr. Wilcke, whoſe treatiſe on the two electricities is admirable, both for its materials and methodical arrangement of them, diſtinguiſhes three cauſes of excitation, viz. *warming*, *liquefaction*, and *friction*; and he adviſes, that we carefully diſtinguiſh between *ſpontaneous* and *communicated* electricity. By the former he means that which is the reſult of the appoſition, or mutual action of two bodies; in conſequence of which, one of them is left poſitively electric and the other negative. Whereas *communicated* electricity is that which is ſuperinduced upon a body, or part of a body, electric or non-electric, without its being previouſly heated, melted, or rubbed; or without any mutual action between it and any other body. This diſtinction is, in general, very obvious; but Mr. Wilcke defines it more accurately than it had been done before, and mentions ſeveral caſes in which they are often confounded.

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* Wilcke, p. 15. and Remarks on Franklin's Letters, p. 270.

SIGNIOR BECCARIA admits the theory of poſitive and negative electricity, though he explains ſome electrical phenomena in a manner different from the other patrons of that ſyſtem.

HE ſuppoſes that electrified bodies move to one another only in the act of giving and receiving the electric fluid *: this effect being produced by the electric matter making a vacuum in its paſſage, and the contiguous air afterwards collapſing, and thereby puſhing the bodies together †. This vacuum, he ſays, is very obſervable upon great exploſions of thunder, when animals have been ſtruck dead without being touched with the lightning; a vacuum being only ſuddenly made near them, and the air immmediately ruſhing out of their lungs to fill it, whereby they are left flaccid and empty; whereas when perſons are properly killed by lightning, their lungs are found diſtended ‡.

IN confirmation of this hypotheſis, he ſays, that leſs motion is given to bodies by electricity, as the air is excluded from them, and that in vacuo no motion at all can be given to them ||. He alſo ſays, that no electric light is viſible in a barometer in which there is a perfect vacuum: whence he infers, that electric light is viſible only by means of ſome vibrations which it excites in the air §.

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* Lettere dell' elettriciſmo, p. 36.

† Ibid. p. 41.

‡ Ibid. p. 42.

|| Ibid. p. 48.

§ Ibid. p. 50.

To account for the collection or diſſipation of electricity by points, he ſays that the electric fluid appears, from experiments, to move with the greateſt violence in the ſmalleſt bodies. All electrical appearances will, therefore, be moſt ſenſible at the points of bodies; and, conſequently, it will be ſooneſt diſſipated there. But this does not ſeem to touch the real difficulty.

Dr. Franklin, the author of this excellent theory of poſitive and negative electricity, with a truly philoſophical greatneſs of mind, to which few perſons have ever attained, always mentions it with the utmoſt diffidence. Every appearance, ſays he, which I have yet ſeen, in which glaſs and electricity are concerned, are, I think, explained with eaſe by this hypotheſis. Yet, perhaps, it may not be a true one, and I ſhall be obliged to him who affords me a better *.

It is no wonder, indeed, that this excellent philoſopher ſhould treat even his own hypotheſis with ſuch indifference, when he had ſo juſt a ſenſe of the nature, uſe, and importance of all hypotheſes. Nor is it, ſays he, of much importance to us, to know the manner in which nature executes her laws. It is enough if we know the laws themſelves. It is of real uſe to us to know that china left in the air, unſupported, will fall and break; but how it comes to fall, and why it breaks, are matters of ſpeculation. It is a pleaſure indeed to know them, but we can preſerve our china without it †.

The great merit of this writer as an electrician ſtands independent of all hypotheſes, upon the firm baſis of the diſcovery of many new and import facts, and, what is more, applied to the greateſt uſes. Suppoſing him, for inſtance, to have been miſtaken in his account how the clouds come to

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* Franklin's Letters, p. 78.

† Ibid. p. 59.

be poſſeſſed of electricity, muſt not all the world acknowledge themſelves indebted to him for the diſcovery of the ſameneſs of the electric fluid and the matter of lightning; and eſpecially for a certain method of preſerving their buildings and perſons from the fatal effects of thunder ſtorms.

SECTION III.

OF THE THEORY OF TWO ELECTRIC FLUIDS.

CONVINCED, as the reader may have perceived that I am, of the uſefulneſs of various theories, as ſuggeſting a variety of experiments, which lead to the diſcovery of new facts; he will excuſe me, if I recall his attention to the old theory of vitreous and reſinous electricity, as it was firſt ſuggeſted by Mr. Du Fay, upon his diſcovery of the different properties of excited glaſs, and excited amber, ſulphur, roſin, &c. and as it has been new modelled by Mr. Symmer. To ſhow my abſolute impartiality, I ſhall, notwithſtanding the preference I have given to Dr. Franklin's theory, endeavour to repreſent this to as much advantage as poſſible, and even to do it more juſtice than has yet been done to it, even by Mr. Symmer himſelf; who, as I obſerved before, has fallen into ſome miſtakes in his application of it. Indeed, hitherto very little pains has been taken with this theory, nor has it been extended to any great variety of phenomena.

LET us ſuppoſe then, that there are two electric fluids, which have a ſtrong chymical affinity with each other, at the ſame time that the particles of each are as ſtrongly repulſive of one another. Let us ſuppoſe theſe two fluids, in ſome meaſure, equally attracted by all bodies, and exiſting in intimate union in their pores, and while they continue in this union to exhibit no mark of their exiſtence. Let us ſuppoſe that the friction of any electric produces a ſeparation of theſe two fluids, cauſing (in the uſual method of electrifying) the vitreous electricity of the rubber to be conveyed to the conductor, and the reſinous electricity of the conductor to be conveyed to the rubber. The rubber will then have a double ſhare of the reſinous electricity, and the conductor a double ſhare of the vitreous; ſo that, upon this hypotheſis, no ſubſtance whatever can have a greater or leſs quantity of electric fluid at different times. The qualityof it only can be changed.

THE two electric fluids, being thus ſeparated, will begin to ſhow their reſpective powers, and their eagerneſs to ruſh into re-union with one another. With whichſoever of theſe fluids a number of bodies are charged, they will repel one another, they will be attracted by all bodies which have a leſs ſhare of that particular fluid with which they are loaded, but will be much more ſtrongly attracted by bodies which are wholly deſtitute of it, and loaded with the other. In this caſe they will ruſh together with great violence.

UPON this theory, every electric ſpark conſiſts of both the fluids ruſhing contrary ways, and making a double current. When, for inſtance, I preſent my finger to a conductor loaded with vitreous electricity, I diſcharge it of part of the vitreous, and return as much of the reſinous, which is ſupplied to my body from the earth. Thus both the bodies are unelectrified, the balance of the two powers being perfectly reſtored.

WHEN I preſent the Leyden phial to be charged, and, conſequently, connect the coating of one of its ſides with the rubber, and that of the other with the conductor, the reſinous electricity of that ſide which is connected with the conductor is tranſmitted to that which is connected with the rubber, which returns an equal quantity of its vitreous electricity; ſo that all the vitreous electricity is conveyed to one of the ſides, and all the reſinous to the other. Theſe two fluids, being thus ſeparated, attract one another very ſtrongly through the thin ſubſtance of the intervening glaſs, and ruſh together with great violence, whenever an opportunity is preſented, by means of proper conductors. Sometimes they will force a paſſage through the ſubſtance of the glaſs itſelf; and, in the mean time, their mutual attraction is ſtronger than any force that can be applied to draw away either of the fluids ſeparately.

Having ſtated the general principles of this hypotheſis of two fluids, I ſhall now enter into a brief compariſon of it with that of a ſingle fluid, as explained by the mode of poſitive and negative electricity; that we may ſee which of them will account for the ſame facts in the eaſier manner and more agreeable to the analogy of nature in other reſpects. For, allowing that no fact can be ſhown to be abſolutely inconſiſtent with either of them; yet, certainly, that will be judged preferable, which is attended with the leaſt difficulty in conceiving of its mode of operation.

In the firſt place, the ſuppoſition itſelf, of two fluids, is not quite ſo eaſy as that of one, though it is far from being diſagreeable to the analogy of nature, which abounds with affinities, and in which we ſee innumerable caſes of ſubſtances formed, as it were to unite with and counteract one another. Here, likewiſe, agreeable to the theory of two electric fluids, while thoſe ſubſtances are in union, we ſee nothing of their ſeparate and peculiar powers, though they be ever ſo remarkable. What, for inſtance, do we ſee of the ſtriking properties of the *acid* and *alkali* while they are united in a neutral ſalt? What powers in nature are more formidable than the vitreolic acid, and phlogiſton, and what more innocent than common ſulphur, which is

a coma compoſition of them both, and from which the action of fire ſeparates them.

THE two fluids being ſuppoſed, the double current from the rubber to the conductor and from the conductor to the rubber is an eaſy and neceſſary conſequence. For if, upon the common ſuppoſition, the action of the rubber puts a ſingle fluid into motion in one direction, we might expect that, if there were two fluids, which counteracted each other, they would, by the ſame operation, be made to move in contrary directions. And a perſon who has been uſed to conceive that a ſingle fluid may be made to move either way, viz. from the conductor to the rubber, or from the rubber to the conductor, at pleaſure, according as a rough or a ſmooth globe is uſed, can have much leſs objection to this part of the hypotheſis.

ADMITTING then this different action of the rubber and the electric upon the two different fluids, the manner of conveying electric atmoſpheres, or powers to bodies is the ſame on this as on any other theory; and it is apprehended, that the phenomena of negative electricity are more eaſily conceived by the help of a real fluid, than by no fluid at all. Indeed Dr. Franklin himſelf ingenuouſly acknowledges, that he was a long time puzzled to account for bodies that were negatively electrified repelling one another; whereas Mr. Du Fay, who obſerved the ſame fact, had no difficulty about it, ſuppoſing that he had diſcovered another electricity, ſimilar, with reſpect to the properties of elaſticity and repulſion, to the former.

BY this double action of the rubber, the method of charging a plate of glaſs is exceedingly eaſy to conceive. Upon this hypotheſis, all the vitreous electricity quits its union with the reſinous on the ſide communicating with the rubber, and is brought over to the ſide communicating with the conductor;

which, by the ſame operation, had been made to part with its reſinous electricity in return.

All the vitreous electricity being thus brought to one ſide of the plate of glaſs, and all the reſinous to the other, the phenomena of the plate while ſtanding charged, or when diſcharged, are, perhaps, more free from all difficulty than upon any other hypotheſis. When one of the ſides of the glaſs is conceived to be loaded with one kind of electricity, and the other ſide with the other kind; the ſtrong affinity between them, whereby they attract each other with a force proportioned to their nearneſs, immediately ſupplies a ſatisfactory reaſon, why ſo little of either of the fluids can be drawn from one of the ſides without communicating as much to the other. Upon this ſuppoſition, that conſequence is perhaps more obvious than upon the ſuppoſition of one half of the glaſs being crowded with the electric matter, and the other half exhauſted. In the former caſe, every attempt to withdraw the fluid from one of the ſides is oppoſed by the more powerful attraction of the other fluid on the oppoſite ſide. On the other hypotheſis, it is only oppoſed by the attraction of the empty pores of the glaſs.

Lastly, the exploſion upon the diſcharge of the glaſs has as much the appearance of two fluids ruſhing into union, in two oppoſite directions, as of one fluid, proceeding only in one direction. The ſame may be ſaid of the appearance of every common electric ſpark, in which, upon this hypotheſis, there is always ſuppoſed to be two currents, one from the electric, or the electrified body, and the other to it.

I do not ſay that the bur which is uſually ſeen on both ſides of a quire of paper pierced by an electric exploſion, and the current of air flowing from the points of all bodies electrified negatively as well as poſitively, are material objections to the doctrine of a ſingle fluid. I have even ſhown how they may be explained in a manner conſiſtent with it; but upon the ſuppoſition of two fluids, and two currents, the difficulty of accounting for theſe facts would hardly have occurred.

THE phenomena of diſcharging a plate of glaſs, upon the hypotheſis of two fluids, are indeed injudiciouſly explained by Mr. Symmer; who ſuppoſes that the two fluids do not always make the whole circuit of the intervening conductors, but enter them, more or leſs, from each ſide of the plate, according to the ſtrength of the charge. But upon this ſuppoſition, the fire of the ſmalleſt charge performs the whole circuit, as well as the fire of the greateſt, in order to reſtore the equilibrium of the two fluids on each ſide of the glaſs.

IT is almoſt needleſs to obſerve, that the influence of points is attended with exactly the ſame difficulty upon this theory as upon the other. It is equally eaſy, or equally difficult, to ſuppoſe one fluid to enter and go out at the point of an electrified conductor, at different times, as to ſuppoſe that, of two fluids, one goes out, and the other goes in, at the ſame time.

THAT bodies immerged in electric atmoſpheres muſt acquire the contrary electricity, is quite as eaſy to conceive upon this, as upon any other hypotheſis. For, in this caſe, ſuppoſe the electrified body to be poſſeſſed of the vitreous electricity, all the vitreous electricity of the body which is brought near it will be driven backwards, to the more diſtant parts, and all the reſinous electricity will be drawn forwards. And when the attraction between the two electricities, in theſe different bodies, is ſo great as to overcome the oppoſition to their union, occaſioned by the attraction of the bodies that contained them, the form of their ſurfaces, and the reſiſtance of the interpoſing medium, they will ruſh together; an electric ſpark will be viſible between them, and the electricity of both will appear to be diſcharged; the prevailing electricity of each being ſaturated with an equal quantity of the oppoſite kind, from the other body.

This hypotheſis will likewiſe eaſily account for the difficulty of charging a very thick plate of glaſs, and the impoſſibility of charging it beyond a certain thickneſs: for theſe fluids at a greater diſtance, will attract one another leſs forcibly; and at a certain greater diſtance will not attract at all.

Having given the moſt favourable view that I can of this hypotheſis of two electric fluids, I ſhall, with the ſame fairneſs, make the beſt anſwer I am able to the principal objection that will probably be made to it.

If it be aſked, why the two fluids, meeting on the ſurface of the globe, or in the electric exploſion, do not unite, by means of their ſtrong affinity, and make no farther progreſs; it may be anſwered, that the attraction between all other bodies and the particles of both theſe fluids may be ſuppoſed to be, at leaſt, as ſtrong as the affinity between the fluids themſelves; ſo that the moment that any body is diſpoſſeſſed of one, it may recruit itſelf, to its uſual point of ſaturation, from the other.

Besides, in whatever manner it be that one of the electric fluids is diſlodged from any body (ſince, upon every theory, the two electricities are always produced at the ſame time) the oppoſite electricity will, by the ſame action, be diſlodged from the other ſubſtance. And (as upon the common theory) whatever it be that diſlodges the fluid from any ſubſtance, it will be ſufficient to prevent its return; conſequently, ſuppoſing both the ſubſtances neceſſarily to have a certain proportion of electric matter, each may be immediately ſupplied from that which was diſlodged from the other.

The rubber, therefore, at the time of excitation, gives its vitreous electricity to that part of the ſmooth glaſs againſt which it has been preſſed, and takes an equal quantity of the reſinous in return. The glaſs, being a non-conductor, does not allow this additional quantity of vitreous electricity to enter its ſubſtance. It is therefore diffuſed upon the ſurface, and, in the revolution of the globe, is carried to the prime conductor. There (as in the experiments begun by Mr. Canton, and proſecuted by Mr. Wilcke, &c.) it repels the vitreous, and violently attracts the reſinous electricity; and (the points of the conductor favouring the mutual tranſition) the vitreous, which abounds upon the globe, paſſes to the conductor; and the reſinous, which abounds on the neareſt parts of the conductor ruſhes upon the globe. There it mixes with, and ſaturates what remained of the vitreous electricity, on the part on which it flows, and thereby reduces it to the ſame ſtate in which it was before it was firſt excited. Every part of the ſurface of the globe performs the ſame office, firſt exchanging electricities with the rubber, and then with the conductor.

THE ſolution of this difficulty will likewiſe ſolve that of the electric exploſion, in which there is a colliſion, as it were, of the two fluids, while yet they completely paſs one another. For ſtill each ſurface of the glaſs may be ſuppoſed to require its certain portion of electric matter, and therefore cannot part with one ſort without receiving an equal quantity of the other. It muſt be conſidered alſo, that the air, through which theſe fluids paſs, has already its natural quantity of electricity, ſo that being fully ſaturated, it can contain no more; and the two fluids only ruſh to the places from whence they had been forcibly diſlodged, and where the greater body of the oppoſite fluid waits to embrace them.

MR. SYMMER's hypotheſis of a double current differed in ſome reſpects from that of the Abbé Nollet. The Abbé, however, according to his uſual candour, ſpeaks of him with the higheſt reſpect; at the ſame time, he ſtill appears an advocate for his old favourite hypotheſis.

MR. CIGNA, who purſued the experiments above recited of Mr. Symmer, obſerves, with reſpect to his theory; that it is not contradicted by any phenomena that are yet known, and that it ſuits ſome of them in a peculiarly clear and elegant manner; particularly every thing relating to charging and diſcharging a plate of glaſs; all the experiments which ſeem to ſhow a mutual attraction between the two electricities, when they are kept aſunder; and that curious experiment above mentioned of Signior Beccaria, of diſcharging a plate of glaſs ſuſpended by a ſilken ſtring, without either touching or moving the plate. Yet, upon the whole, he declares in favour of Dr. Franklin's theory of poſitive and negative electricity, on account of its admirable ſimplicity, and becauſe philoſophers ought not to multiply cauſes without neceſſity.

DR. FRANKLIN'S theory, he ſays, completely ſolves all the caſes of the two electricities deſtroying one another when they are mixed; but doth not ſo clearly account for their attracting, and counteracting one another when they are ſeparate. He concludes with ſaying, he doth not chuſe to ſay much on ſo very obſcure a queſtion, which has divided the opinions of very great men; and that any hypotheſis of the two electricities, which will account for the deſtruction of all the ſigns of electricity when they are united, and their mutual attraction when they are ſeparate, will equally ſuit all the phenomena that have yet been diſcovered.

I HAVE taken a little pains with this theory, becauſe I thought it had been, hitherto, too much overlooked, and that ſufficient juſtice had not been done to it, even by thoſe who propoſed it. For the future, I hope it will be ſeen to more advantage, and appear a little more reſpectable among its ſiſter hypotheſes;

and then, *valeat quantum valere poteſt.* If any electrician will favour me with the communication of any other theory, not obviouſly contradicted by facts, I ſhall think myſelf obliged to him, and ſhall think I do a piece of real ſervice to the ſcience in the publication of it. If more perſons favour me with more different theories, I ſhould think my book, as far as theories are of any uſe, ſo much the more valuable.

PART IV.

DESIDERATA IN THE SCIENCE OF ELECTRICITY, AND HINTS FOR THE FARTHER EXTENSION OF IT.

SECTION I.

GENERAL OBSERVATIONS ON THE PRESENT STATE OF ELECTRICITY.

THAT real progreſs has been made in electricity, has, I preſume, been ſufficiently demonſtrated in the courſe of the preceding hiſtory; that a great deal ſtill remains to be done, will, I think, be evident from this part of the work. Thoſe perſons who think that nothing has been done to any purpoſe in Natural Philoſophy, or that the advances have been made very ſlowly, ſince the time of Sir Iſaac Newton, need only read the preceding hiſtory, to be convinced, both that a great deal has been done, and that the progreſs in this kind of knowledge, inſtead of being ſlow, has been amazingly rapid. To quicken the ſpeed of philoſophers in purſuing this progreſs, and at the ſame time, in ſome meaſure, to facilitate it, is the intention of this treatiſe, and more eſpecially of this part of it. When a traveller imagines he is near his journey's end, he is little ſolicitous about making diſpatch, thinking that, without any haſte, the labour of the day will quickly be over; whereas, if he find that, whatever progreſs he may have made, he has a great deal ſtill to make, he continues, or quickens his ſpeed.

THE principal reaſon why many ingenious perſons have ſo ſoon got to their *ne plus ultra* in philoſophical diſcoveries, has evidently been their attachment to favourite theories; which they imagined both accounted for all the phenomena that had been obſerved, and would likewiſe account for all that ſhould be obſerved. Having therefore attained to the great object of a ſcience, and diſcovered the ultimate and moſt general principles of it, there was nothing more that was worth their notice; it being beneath men of genius to ſpend their time in diverſifying effects, when there were no new cauſes to be found. I hope that what has hitherto been ſaid concerning the nature and uſe of hypotheſes, and about the progreſs and preſent imperfect ſtate of thoſe which reſpect electricity, will convince thoſe electricians who may not yet have been convinced of it, that our buſineſs is ſtill chiefly with *facts*, and the *analogy of facts*; that far too few of theſe have been diſcovered to aſcertain a perfect general theory, and that all that the preſent hypotheſes can do for us muſt conſiſt in ſuggeſting farther experiments.

IF we look back upon the hiſtory of electricity, and conſider the ſtate of facts and of hypotheſes at any particular period of time paſt, we ſhall ſee that there was always the ſame apparent reaſon for acquieſcing in what had been done, as at preſent. The theories of the firſt electricians, lame and imperfect as they were, were yet ſufficient to account for all the facts they were acquainted with; and as for other facts, they could have no idea or apprehenſion of them, and therefore could not be ſolicitous about them.

Mr. Boyle, no doubt, was as fully ſatisfied with his ſimple hypotheſis of the unctuous effluvia, as Mr. Nollet with his theory of affluences and effluences; or the greateſt part of the preſent race of electricians with that of poſitive and negative electricity. Mr. Haukſbee, when he made his ſurpriſing diſcoveries concerning the properties of electric light, and many curious circumſtances concerning electric attraction and repulſion, might very naturally think that little more was to be done. Indeed, who could have thought otherwiſe, when the ſcience was actually at a ſtand for ſeveral years after him? All that the indefatigable Mr. Grey (who made the great diſcovery of the communication of electric powers to bodies not electric per ſe) imagined to remain undone, were mere chimeras and illuſions. Mr. Du Fay, who made the diſcovery of vitreous and reſinous electricity, had no idea of the electric ſhock; and the German philoſophers, who accidentally obſerved it, knew nothing of its moſt remarkable properties. Notwithſtanding a great number of treatiſes on the ſubject of electricity appeared preſently after this diſcovery, and ſome of them very ſyſtematical, comprehending, no doubt, what the authors of them thought to be the whole of the ſcience, yet none of them had the leaſt idea of the amazing diſcoveries of Dr. Franklin, relating either to the Leyden phial, or to the nature of lightning. And though numbers of Dr. Franklin's admirers thought that he had exhauſted the whole ſubject, he himſelf was far from thinking ſo; and the hiſtory of electricity, ſince the date of his capital diſcoveries, demonſtrates that his ſuſpicion was true.

It may be ſaid, that there is a *ne plus ultra* in every thing, and therefore in electricity. It is true: but what reaſon is there

to

to think that we have arrrived at it. Mr. Grey might have uſed the ſame language above twenty years ago; but every body will now acknowledge, that it would have been above twenty years too ſoon: and yet, I think, it is evident, that Mr. Grey had really more reaſon to think he had arrived at the *ne plus ultra* of electricity, than we have to think that we are arrived at it. Time has brought to light a great number of *incomplete*, as well as complete experiments, and perhaps more of the former than of the latter; concerning all which, as he could have no knowledge, ſo he could have no doubts; ſo that, though we know much more than he did, we, at the ſame time, know how much more is unknown better than he could. Hitherto the acquiſition of electrical knowledge has been like the acquiſition of riches: the more we poſſeſs, the more we wiſh to poſſeſs; and, I hope, the more indefatigable we ſhall be to acquire the poſſeſſion of it.

ONE thing extremely uſeful to the progreſs of farther diſcoveries, is to know what has really been done by others, and where the ſcience ſtands at preſent. For want of this knowledge, many a perſon has loſt his time upon experiments which he might have known had either failed or ſucceeded with others; and which it was, therefore, not worth his while to repeat. But the ſources of this kind of information are too much ſcattered, and too diſtant for moſt perſons to have acceſs to them. This was the firſt motive of the preſent undertaking, intended to exhibit a diſtinct view of all that has been done in electricity to the preſent time, and likewiſe the order and manner in which every thing has been done; that electricians, having a diſtinct idea of what the progreſs of electrical knowledge has been, might ſee more clearly what remains to be done, and what purſuits beſt promiſe to reward their labour.

Indeed it is almoſt impoſſible for any perſon to read the hiſtory of electricity without gathering many hints for new experiments. When he has the whole before him at one view, he can better bring the diſtant parts together; and from the compariſon of them, new lights may ariſe. When he ſees what experiments have failed, and what have ſucceeded; what branches of the ſcience have been moſt attended to, and what things ſeem to have been overlooked; what has been diſcovered by accident, and what by theory; when he ſees both the true lights which directed ſome happy diſcoverers, and the falſe lights which miſled others, he will have the beſt preparation for purſuing his own inquiries.

To point out many of the *deſiderata* in the ſcience of electricity, I am ſenſible, will, for this reaſon, be ſuperfluous to many perſons, and probably to moſt who will have read thus far of this treatiſe: for ſufficient hints of them muſt have been ſuggeſted by the perſual of the hiſtory. But if I have been anticipated in this part of my work by ſome of my readers, it will not diſpleaſe them to find it; and to others, the contents of this chapter will be peculiarly uſeful.

If, indeed, I had conſulted my reputation as a writer, or a philoſopher, I ſhould not have attempted this chapter at all. For not only will many of the articles which I ſhall now put down as *deſiderata* in the ſcience be ſoon no longer ſo, and even young electricians be able to give ſatisfactory anſwers to ſome difficult queries I am going to propoſe; but many of them will probably appear idle, frivolous, or extravagant ones; and, in a more advanced ſtate of the ſcience, it will hardly be imagined why I put them down at all. But if this chapter be a means of haſtening ſo deſirable an event, and of accelerating the progreſs of electrical knowledge, I am very willing that it ſhould, ever after, ſtand as a monument of my preſent ignorance.

"These thoughts," to adopt the words of Dr. Franklin, with much more propriety than he himſelf firſt uſed them, "are many of them crude and haſty; and if I were merely "ambitious of acquiring ſome reputation in philoſophy, I ought "to keep them by me, till corrected and improved by time and "farther experience. But ſince even ſhort hints and imperfect "experiments, in any new branch of ſcience, being communi"cated, have often times a good effect, in exciting the attention "of the ingenious to the ſubject, and ſo become the occaſion "of more exact diſquiſitions, and more complete diſcoveries; "you are at liberty," ſays he, to Mr. Collinſon, "to commu"nicate this paper to whom you pleaſe, it being of more "importance that knowledge ſhould increaſe, than that your friend "ſhould be thought an accurate philoſopher."

I would not even propoſe to draw up the following *queries* upon the plan of thoſe of Sir Iſaac Newton, at the end of his treatiſe on Optics. Many of them are ſuch, that I have hardly the moſt diſtant expectation of their being verified; but the attempt to verify them may poſſibly lead to ſome other diſcoveries of more importance. They are ſuch random thoughts as led to the new experiments I have made; and not having any more leiſure to purſue them myſelf, I freely impart them to my reader, that he may make as much advantage of them as he can: being determined, upon taking leave of the ſubject, *to write myſelf fairly out*, as Mr. Addiſon ſays; or, as the Spaniſh writers ſay, *to leave nothing in my inkhorn*.

Happy would it be for ſcience, if all philoſophers who are engaged in the ſame purſuits, would make one common chapter of all their hints and queries: and greatly honoured ſhould I think myſelf if the preſent chapter in this treatiſe might be made uſe of for that purpoſe, and if, in future editions of the work, it ſhould be looked into as the common receptacle of the *preſent deſiderata* among the whole body of electricians, and of their imperfect hints for new diſcoveries. With pleaſure ſhould I ſee each of them diſtinguiſhed by the name of ſome generous and illuſtrious contributor. A few, the reader will find, have been added to my own, and are diſtinguiſhed in this manner.

Many perſons can throw out hints, who either have not leiſure, or a proper apparatus for purſuing them: others have leiſure, and a proper apparatus for making experiments, but are content with amuſing themſelves and their friends in diverſifying the old appearances, for want of hints and views for finding new ones. By this means, therefore, every man might make the beſt uſe of his abilities for the common good. Some might ſtrike out lights, and others purſue them; and philoſophers might not only enjoy the pleaſure of reflecting upon their own diſcoveries, but alſo upon the ſhare they had contributed to the diſcoveries of others.

SECTION II.

QUERIES AND HINTS CALCULATED TO PROMOTE FARTHER DISCOVERIES IN ELECTRICITY.

I.

QUERIES AND HINTS CONCERNING THE ELECTRIC FLUID.

WHAT is the proportion of the ſeveral colours in electric light, in different caſes, and in different appearances of it ?

Is not the electric light a real vapour ignited, ſimilar to that of phoſphorus ; and may not experiments be, hereafter, made, where we ſhall have the exploſion, the ſhock, and the other effects of electricity, without the light ? Is the electric light ever viſible except in vacuo ? In the open air the electric fluid makes itſelf a vacuum in order to its paſſage.

COLLECT the electric fluid, not from the general maſs of the earth but from bodies of particular kinds, and obſerve if it have any different properties, with reſpect to light, &c.

Is it exactly the ſame at ſea, as on land ; below the ſurface of the earth, as above it, &c. &c. &c. ?

DR. FRANKLIN obſerved, that iron was corroded by being expoſed to repeated electric ſparks. Muſt not this have been effected by ſome acid? What other marks are there of an acid in the electric matter? May not its phoſphoreal ſmell be reckoned one?

Is there only one electric fluid, or are there two? Or is there any electric fluid *ſui generis* at all, diſtinct from the ether of Sir Iſaac Newton? If there be, in what reſpect does it differ from the ether?

ARE the particles which affect the organ of ſmelling, as well as the particles of light, parts of the proper electric fluid, or are they merely adventitious, being, ſome way or other, brought into action by electricity?

DOES not ſome particular order of the particles, which Sir Iſaac Newton ſuppoſes to be continually flying from the ſurfaces of all bodies, conſtitute the electric fluid; as others, he imagined, conſtitute the air, and others the ether, &c.?

Is it probable that there is even any temporary, or growing addition to, or diminution of the whole ſtock of electricity?

WHENCE ariſes the elaſticity of the electric fluid, and according to what law do its particles repel one another? *Mr. Price.*

Is not the electric matter the ſame with, or at leaſt, hath it not ſome near relation to that luminous matter which forms the ſolar atmoſphere, and produces the phenomenon called the *zodiacal light*; which is thrown off principally, and to the greateſt diſtance, from the equatorial parts of the ſun, in conſequence of his rotation on his axis, extending viſibly in the form of a luminous pyramid, as far as the orbit of the earth; and which, according to Monſieur De Mairan's ingenious, and at leaſt, plauſible hypotheſis, falling into the upper regions of our atmoſphere, is collected chiefly towards the polar parts of the earth, in conſequence of the diurnal revolution, where it forms the aurora borealis?

MAY not the ſun be the fountain of the electric fluid; and the zodiacal light, the tails of comets, the aurora borealis, lightning, and artificial electricity as its various, and not very diſſimilar modifications*?

DID not the ſulphureous ſmell draw our attention towards the vitreolic acid, the peculiar hiſſing noiſe accompanying the electric blaſt, ſpontaneouſly iſſuing, for inſtance, from the pointed wire of a fully charged phial, appears rather to mimic the exploſive action of deflagrating nitre; and may, accordingly, without much violence, be ſuppoſed to ariſe from the nitrous aerial acid, violently commenſtruating with the phlogiſton, which it either meets with in the air, or which is conveyed to it by the electrified body. Or, were we to adopt the hypotheſis of two diſtinct electric fluids, we might, by way of temperament, propoſe as a query, whether the nitrous acid, &c. may not be the conſtant concomitant of thoſe exploſive pencils of light, which are obſerved to dart from the points of bodies replete with the vitreous electricity; while the ſilent and languid luminous ſpecks (reſembling the ſmall tip of a lighted match) appearing on the extremities of bodies endued with a reſinous electricity, may as probably indicate the accenſion of a ſulphureous matter, and conſequently the preſence of the vitreolic acid, the electric ſpark of exploſion, appearing on the approach of the two bodies, being conſidered as the effect, at leaſt, in part of the menſtrual action of theſe two acids on each other, &c.†

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* Monthly Review, October 1767, p. 353.

† Ibid. December 1767, p. 458.

II.

Queries and hints concerning ELECTRICS and CONDUCTORS.

In what does the difference between electrics and conductors conſiſt? In other words, what is it that makes ſome bodies permeable to the electric fluid, and others impermeable to it?

Are the pores of electric bodies ſmaller than thoſe of conductors, and do they contain very much, or very little of the electric fluid?

What is it in the internal ſtructure of bodies that makes them break with a poliſh? Perhaps all ſolid electrics do ſo.

Has elaſticity any connection with electricity, ſome electrics being extremely elaſtic?

What is the reaſon why, in ſome of Mr. Haukſbee's experiments, the electric light was viſible through a conſiderable thickneſs of very opaque electrics, as roſin, ſulphur, pitch, &c. but not through the thinneſt metallic conductors?

What ſimilarity is there in the proceſſes of calcination, vegetation, animalization, and in ſome meaſure cryſtalization; ſince all bodies which have gone through any of thoſe proceſſes, and perhaps no others, are found to be electrics?

Are not both electrics and conductors more perfect in their kind in proportion to their ſpecific gravity?

Will not water conduct electricity the beſt in its ſtate of greateſt condenſation; and metals the leaſt in their greateſt expanſion, as ſhown by a pyrometer?

COMPARE the inviſible effluvia of water with the inviſible effluvia of a burning candle, and alſo thoſe proceeding from other bodies, with reſpect to their power of conducting electricity.

OBSERVE what degree of heat will diſcharge any given degree of electricity, in order to find in what degree heat makes air a conductor.

III.

QUERIES AND HINTS CONCERNING EXCITATION.

WHAT is the difference, in the internal ſtructure of electrics, that makes ſome of them excitable by friction, and others by heating and cooling?

WHAT have friction, heating, cooling, and the ſeparation after loſe contact in common to them all? How do any of them contribute to excitation? And in what manner is one, or the other kind of electricity produced by rubbers and electrics of different ſurfaces?

Is not Mr. Æpinus's experiments of preſſing two flat pieces of glaſs together, when one of them contracts a poſitive and the other a negative electricity, ſimilar to the experiments of Mr. Wilcke concerning the production of electricity by the liquefaction of various ſubſtances in others; when the ſubſtance which melts and contracts is in one ſtate, and that which contains it is in the oppoſite? And are not both theſe caſes ſimilar to the excitation of the tourmalin, &c. by heating and cooling? In this caſe may not the tourmalin and the air act upon one another and be in oppoſite ſtates?

Is not the circumſtance common to all theſe caſes, ſome affection of that ſpace near the ſurface of the bodies in which the refractive power lies? When bodies which have been preſſed together within that ſpace recede from one another, more ſurface, and conſequently more of that ſpace is made, doth not the electric fluid flow into it from that body which has the leaſt power of retaining it, and which it can permeate with the moſt eaſe; when not being able to enter the ſubſtance of the other, it reſts upon its ſurface?

Are not the particles of the electric and rubber thrown into a vibration in the act of excitation, which makes frequent recedings of the parts from one another, and thereby promotes the effect above mentioned?

What is the real effect of putting moiſture or amalgam upon the rubber? Do not thoſe ſubſtances increaſe the power of excitation, as conductors more diſtant from the ſmooth glaſs, in the gradation of electrics, than the ſurface of the leather? Or do they only make the rubber touch in more points, or alter the ſurface of the rubber?

Has that difference of ſurface on which colour depends, any influence upon the power of excitation?

The tourmalin and a veſſel of charged glaſs hermetically ſealed are both excited by heating and cooling. What other properties have they in common?

IV.

QUERIES AND HINTS CONCERNING ELECTRIFICATION.

DOES electrification increaſe the exhalation of vapours either from cold or from boiling water? If it do, is the increaſed exhalation the ſame in all ſtates of the atmoſphere?

DOES not the electric matter paſs chiefly on the ſurface of bodies?

IS the action of electrified bodies upon one another more properly an attraction or a repulſion?

WOULD not continued electrification promote putrefaction?

IN what manner is the mutual repulſion of two bodies electrified negatively performed? Is it by the attraction of the denſer electric fluid in the neighbourhood, by the quantity of it which may be ſuppoſed to be accumulated on the ſurfaces of ſuch bodies in the manner deſcribed p. 420, or to the mutual repulſion of the particles of matter of which the bodies conſiſt?

V.

QUERIES AND HINTS CONCERNING THE POWER OF CHARGING ELECTRICS.

WHAT is the real operation of conductors in coating electric ſubſtances?

WHAT is the *maximum* of charging a glaſs jar, with reſpect to the quantity of its ſurface, covered by the coating? It is evident that ſome jars will diſcharge themſelves, when only a ſmall part at the bottom of them is coated, and when the exploſion is very inconſiderable.

ENDEAVOUR to charge a plate of glaſs with the coating preſſed into actual contact with its ſurface, by means of heavy weights. Alſo endeavour to excite a plate of glaſs in the ſame manner. It is pretty certain that, in the uſual method of exciting and charging, the real ſubſtance of the glaſs is not touched; and though water be attracted by glaſs; it may only be to a certain diſtance from it.

VI.

QUERIES CONCERNING THE ELECTRICITY OF GLASS.

THROUGH what thickneſs of glaſs will an excited electric, of any given ſtrength, attract and repel light bodies? Is not the ſame thickneſs the limit of charging glaſs with the electric fluid?

Is not a plate of glaſs contracted in its dimenſions by charging, the two electricities ſtrongly compreſſing it, ſo as to increaſe its ſpecific gravity?

Is the tone of a glaſs veſſel, made in the form of a bell, the ſame when it is charged as when it is uncharged? Or would the ringing of it make it more liable to break in thoſe circumſtances?

DOES the electric fluid with which glaſs is charged reſide in the pores of the glaſs, or only on its ſurface; or rather within the ſpace that is occupied by the power of refraction, i. e. a ſmall ſpace within, and likewiſe without the ſurface?

Is the refractive power of glaſs the ſame when it is charged or excited?

How does the different refractive power of glaſs, or its denſity (which is probably in the ſame proportion with its refractive power) affect its property of being excited or charged?

Is there not a conſiderable difference in glaſs when it is new made, and when it has been kept a month or two, both with reſpect to excitation and charging?

LET glaſs of every different compoſition be tried both with reſpect to excitation and charging. Would it not be found that differences with reſpect to metallic ingredients, hardneſs, annealing, continuance in fuſion, &c. would influence both the properties; and that, in ſeveral caſes, the ſame circumſtance that was favourable to one would be unfavourable to the other?

GLASS has hitherto been ſuppoſed to be full of the electric fluid, and its impermeability has been accounted for upon the difficulty with which the electric fluid moves in its pores. But may we not ſuppoſe the ſubſtance of glaſs to be abſolutely impermeable to electricity, that no foreign electric matter ever ſo much as enters a ſingle pore of it, but lodges wholly on its ſurface; for inſtance, between the point of contact and the real ſurface, or within the limits of the refractive power; that is, a little way on both ſides the ſurface This place is, I think, on many accounts, extremely convenient to diſpoſe of the electric matter, whether we make it to conſiſt of two fluids, or of one. Their being kept aſunder, if there be two, or its being prevented from getting through, if there be but one, will be much eaſier to conceive in this caſe, than upon the ſuppoſition that the electric fluid *can* enter and move in the ſubſtance of the glaſs, though it can only enter and move with difficulty, as Mr. Æpinus expreſſes it. For, let the motion be ever ſo difficult, one would think that this circumſtance could only make it move ſo much the ſlower, and that, give the electricity in the charged plate of glaſs time enough, and it would at length, without any external communication, perform the journey to the other ſide, whither it has ſo ſtrong a tendency to go.

Moreover, one would think, that, upon the hypotheſis of the admiſſion of the electric fluid within the pores of the glaſs, when the diſcharge of a phial was actually made through the ſubſtance of the glaſs, it might be in a ſilent manner, without breaking the glaſs; whereas when the ſurfaces of the glaſs are ſuppoſed to be violently preſſed, and the pores of it not in the leaſt entered by any particle of the fluid, or fluids, the impoſſibility of the electric charge getting through the glaſs is evident, as well as the neceſſity of its breaking the glaſs, if it do force a paſſage.

VII.

Queries and hints concerning the effect of ELECTRICITY on ANIMAL BODIES.

Is the fluid on which electricity depends, at all concerned in any of the functions of an animal body? In what manner is the pulſe of a perſon electrified quickened, and his perſpiration increaſed?

May not the increaſed perſpiration of an animal body be greater in a moiſt atmoſphere than in a dry one, there being then more conducting particles in the atmoſphere, to act and react upon the effluvia in the pores of the body; on which the copious perſpiration does, probably, in a great meaſure, depend?

VIII.

QUERIES AND HINTS CONCERNING THE ELECTRICITY OF THE ATMOSPHERE.

IN what manner do the clouds become poſſeſſed of electricity?

DOES the wind in any meaſure contribute to it?

Is it effected by the gradual heating and cooling of the air? If ſo, whether is it the heating or the cooling that produces poſitive electricity? Which ever it be, the contrary will probably produce negative electricity. Let the experiment be made by an electrical kite. *Mr. Canton.*

As thunder generally happens in a ſultry ſtate of the air, when it ſeems repleniſhed with ſome ſulphureous vapours; may not the electric matter then in the clouds be generated by the fermentation of ſulphureous vapours with mineral or acid vapours in the air? *Mr. Price.*

MR. WILCKE ſuppoſes the air to contract its electricity, in the ſame manner as ſulphur and other ſubſtances do, when they are heated and cooled in contact with various bodies. Thus the air, being heated or cooled in the neighbourhood of the earth, gives electricity to the earth, or takes it from it, and the electrified air, being conveyed upwards, by various means, communicates its electricity to the clouds *.

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* Remarks on Franklin's Letters, p. 302.

Let rain, ſnow, and hail be received in inſulated veſſels, in different ſtates of the atmoſphere, to obſerve whether they contain any electricity, and in what degree.

May not the void ſpace above the clouds be always occupied with an electricity oppoſite to that of the earth? And may not thunder, earthquakes, &c. be occaſioned by the ruſhing of the electric fluid between them, whenever the redundancy in either is exceſſive? Is not the aurora borealis, and other electrical meteors, which are remarkably bright and frequent before earthquakes, ſome evidence of this?

Is not the earth in a conſtant ſtate of moderate electrification, and is not this the cauſe of vegetation, exhalation, and other the moſt important proceſſes in nature? Theſe are promoted by increaſed electrification. And is it not probable that earthquakes, hurricanes, &c. as well as lightning, are the conſequence of a too powerful electricity in the earth?

Supposing earthquakes to be cauſed by the diſcharge of a redundant electricity from the ſurface of the earth, might they not be prevented, in countries ſubject to them, by kites conſtantly flying very high, with wires in the ſtrings, ſo as to promote an eaſy communication between the earth and the upper regions of the atmoſphere?

SECTION III.

BRANCHES OF KNOWLEDGE PECULIARLY USEFUL TO AN ELECTRICIAN.

IN the hiſtorical part of this work I have ſhown what has been done on the ſubject of electricity, and under the preceding *deſiderata*, I have endeavoured to give ſome idea of what yet remains to be done, with a few hints concerning farther experiments. In the cloſe of this part, I would willingly do ſomething more towards enabling my reader to make farther advances in electrical inquiries. However, all that can be done in this way muſt, in its own nature, be more imperfect than even the account of the *deſiderata:* for it is evident, that he who is able to teach others to make diſcoveries might make them himſelf. Notwithſtanding this, it is poſſible that ſome general obſervations may be of uſe to this purpoſe; ſuch for inſtance as Lord Bacon makes, in his *Novum Organon*; a book which, though it contain few or no philoſophical diſcoveries itſelf, has contributed not a little to the diſcoveries contained in others. A few ſuch general obſervations, confined to the ſubject of electricity, I ſhall endeavour to ſuggeſt in this place.

IT is an obſervation which the progreſs of ſcience daily confirms, that all truths are not only conſiſtent, but alſo connected with one another. The obſervation has, with no ſmall appearance of juſtice, been extended even to the arts; there being no two of them ſo remote, but that ſome of the methods and proceſſes uſed in the one have ſome analogy to ſome that are uſed in the other. Hence the knowledge of one art or ſcience is ſubſervient to the knowledge of others; and no perſon can preſume that he is perfectly maſter of any one, till he has received all the aſſiſtance he can from, at leaſt, all its ſiſter arts or ſciences.

Indeed the very exiſtence of the various arts and ſciences is almoſt a demonſtration of their relation to each other. For it were highly unreaſonable to ſuppoſe, that the elements of any new art or ſcience were diſcovered by means independent of the ſtudy or practice of thoſe already known. As it is by eaſy tranſitions that we paſs from one part of any particular ſcience to another, ſo it is by tranſitions equally eaſy that mankind have paſſed from one diſtinct ſcience to another. Conſequently, to thoſe previouſly diſcovered arts and ſciences muſt we have recourſe, in order to underſtand the full evidence, on which the firſt principles of any new art or ſcience reſt.

Electricity is by no means an exception to this general rule. It has its ſiſter ſciences as well as others. In purſuance of them were its own principles firſt diſcovered; and the farther proſecution of electrical experiments has ſhown its connection with more ſciences than it was at firſt apprehended to have any relation to. Now the ſtudy of all theſe cannot but, reciprocally, contribute to perfect and extend the knowledge of electricity.

Gilbert, the firſt of modern electricians, was led to make his electrical experiments by their relation to thoſe of magnetiſm, into which he was profeſſedly inquiring. The ſtudy of chymiſtry ſeems to have led Mr. Boyle to attend to electricity, as well as to other occult qualities of particular bodies. Electric light was conſidered by all thoſe who firſt obſerved it as a ſpecies of phoſphorus; and with this view was Mr. Haukſbee conducted in all the experiments he made upon it.

THESE, and other diſcoveries in electricity, having been made thus indirectly, excited the attention of philoſophers to the ſubject, and induced them to ſit down to the ſtudy of it in a direct and profeſſed manner. Upon this it ſoon appeared, that electricity was no ſecondary, or occaſional, but a principal, and conſtant agent in the works of nature, even in ſome of its grandeſt ſcenes; and that its agency, far from being confined to bodies of a particular claſs, extended its influence to all without exception; that the mineral, vegetable, and animal world, with the human frame in a particular manner, were all ſubject to its power; and that electrical experiments and principles enter into the moſt intereſting arts and ſciences which have them for their object. We alſo ſee every day, that electricity is extending itſelf ſtill more into the ſubjects of other ſciences, both by means of the analogy of their operations, and alſo by their reciprocal influences.

ON theſe accounts, to be an electrician at preſent, requires a much more extenſive fund of various knowledge than it did but ten years ago; and a man muſt have a very comprehenſive knowledge of nature in all its known operations, before he can reaſonably expect to make any farther diſcoveries. For it can only be by applying electricity to various parts of nature, and by combining its operations with other operations, both of nature and art, that any thing new can be found out. Almoſt all that can be done by the common electrical machines, and the uſual apparatus of them, has been done already; ſo that we muſt look farther in queſt of new diſcoveries. I hope, therefore, that I ſhall be excuſed, if I endeavour to give a hint of that kind of knowledge which, I apprehend, may be peculiarly ſubſervient to improvements in electricity, and furniſh views and materials for new experiments.

NATURAL PHILOSOPHY cannot but be of the greateſt uſe for this purpoſe; but, of all its branches, none promiſes to be of more uſe to the electrician than CHYMISTRY. Here ſeems to be the great field for the extenſion of electrical knowledge: for chymiſtry and electricity are both converſant about the latent and leſs obvious properties of bodies; and yet their relation to each other has been but little conſidered, and their operations hardly ever combined; few of our modern electricians having been either ſpeculative or practical chymiſts.

AMONG other branches of Natural Philoſophy, let the doctrine of LIGHT AND COLOURS be alſo particularly attended to. It was this that Newton thought would be the key to other, at preſent, occult properties of bodies.

LET particular attention be alſo given to every thing that the imperfect ſtate of Natural Philoſophy furniſhes reſpecting the ATMOSPHERE, its compoſition and affections. The phenomena of lightning ſhow the connection of this ſubject with electricity; and, probably, electricity may be our key to a much more extenſive knowledge of meteorology than we are yet poſſeſſed of.

THE ſhock of the Leyden phial, the diſcovery of the ſameneſs of lightning and electricity, together with the cure of ſeveral diſeaſes by electrical operations, are ſufficient to convince us of the ſingular importance of the ſtudy of ANATOMY, and every thing relating to the animal œconomy to an electrician. And had phyſicians more generally attended to electricity, as an article of the materia medica, many more important and uſeful diſcoveries might, no doubt, have been made. Enow, however, have been made to excite us to farther inquiries.

MR. ÆPINUS has lately given us an excellent ſpecimen of what uſe MATHEMATICS, and eſpecially algebraical calculations, may be to an electrician; and their uſe will probably, in time, be found ſtill more extenſive.

As electricity has much to expect from ſeveral branches of Natural Philoſophy, ſo it will be ready, in its turn, to lend its aſſiſtance to them. It already ſupplies arguments and proofs of ſome principles in Natural Philoſophy, which ſtrengthen thoſe that are drawn from other quarters. By electricity, as well as by the principles of light and colours, we can demonſtrate, that it requires a conſiderable force to bring bodies which are contiguous to one another, and even lie upon one another, into actual contact; and the moiſture of the air may perhaps be ſhown to more exactneſs by Mr. Canton's electrical balls than by any other hygrometer whatever. But I do not mean to purſue this ſubject, and only mention theſe caſes by way of example.

UPON the ſubject of the proper furniture for an electrician, I think it may be juſtly added, that a knowledge of MECHANICS will be uſeful to him; by which I mean, upon this occaſion, not only the theory, but in ſome meaſure the practice too. For without ſome mechanical knowledge of his own his electrical machinery will be very often out of order, and but ill anſwer his purpoſe.

IF, indeed, a perſon mean nothing more than to amuſe himſelf and his friends with the experiments that have been made by others (and this is a method of amuſement which I am far from diſcouraging) the machines he may purchaſe, ready conſtructed to his hands, will anſwer his purpoſe very well; and the directions which are uſually given along with the machines will enable him to perform the common experiments with tolerable certainty: and if any damage ſhould happen to his apparatus, a mathematical inſtrument-maker (if he happen to live in or near a large town) can readily repair it for him. But if a man propoſe to ſtudy the ſubject of electricity as a philoſopher, with a view to extend the knowledge of it, the aſſiſtance of others will not be ſufficient for him.

The common electrical machines, and the uſual electrical apparatus, will enable a perſon to do little more than exhibit the common experiments. If he propoſe to go farther, he muſt diverſify his apparatus; he muſt often alter the conſtruction of his machines, and will find that common workmen cannot execute any thing out of their uſual way, without more than general directions. Beſides, unleſs a perſon be fortunately ſituated, workmen of every kind cannot always be at hand, to do every little thing he may want in the mechanical way, whenever he may happen to get a hint of a new experiment that requires it.

An electrician, therefore, ought never to be without the common tools of a *cabinet-maker*, *clock*, and *watch-maker*, at leaſt, and know, in ſome meaſure, how to uſe them. With reſpect to glaſs, he ought, by all means, to learn the uſe of a *blow-pipe*, the method of drawing out and bending glaſs tubes, and performing, with ſome degree of dexterity, other operations upon glaſs, which he will want to uſe in a great variety of forms. An electrician, thus furniſhed, will be able, upon any occaſion, to ſerve himſelf: and the ſlowneſs and blunders of mechanics do but ill ſuit with the ardour of perſons engaged in philoſophical inquiries.

It were much to be wiſhed, that philoſophers would attend more than they do to the conſtruction of their own machines. We might then expect to ſee ſome real and capital improvements in them; whereas little can be expected from mere mathematical inſtrument makers; who are ſeldom men of any ſcience, and whoſe ſole aim is to make their goods elegant and portable.

Formerly, indeed, philoſophers were obliged to conſtruct their own machines. Mr. Boyle, Mr. Haukſbee, and Dr. Deſaguliers would have done nothing by giving tradeſmen orders for what they wanted. There were no ſuch things to had.

Neceſſity therefore drove them to the ſtudy and practice of mechanics, and from their contrivances are derived almoſt all the philoſophical inſtruments which are now in uſe.

EVERY original genius, like them, muſt, in this reſpect, follow their ſteps. He will extend his views beyond the power of the preſent machinery, which can only be adapted to the preſent ſtate of ſcience. And, I think, one principal reaſon of the imperfect ſtate of ſeveral branches of electrical knowledge with us, may be evidently traced to ſome general imperfections in the ſtructure of all our common machines in England; which render ſeveral kinds of experiments very difficult, or almoſt impoſſible to be made; as may be ſhown in the next part of the work, in which I ſhall treat at large of the conſtruction of machines, and give the beſt directions I am able for uſing them.

LASTLY, if an electrician intend that the public ſhould be benefited by his labours, he ſhould, by all means, qualify himſelf to draw according to the rules of PERSPECTIVE; without which he will often be unable to give an adequate idea of his experiments to others. There is ſo much beauty in the rules of this ingenious art, and ſo much pleaſure in the application of them, that I cannot help wondering, that all gentlemen of a liberal education do not take the ſmall degree of pains, that is neceſſary to make themſelves maſters of it. All the mechanical methods of drawing, eſpecially where a great number of right lines are uſed, as in drawing machines, &c. are exceedingly imperfect, and inſufficient. They admit not of half the variety of perſpective drawings. They can hardly ever be near ſo correct; beſides that, I know by experience, they take up much more time, and the operation is exceedingly ſlaviſh and troubleſome.

PART V.

OF THE CONSTRUCTION OF ELECTRICAL MACHINES, AND THE PRINCIPAL PARTS OF AN ELECTRICAL APPARATUS.

SECTION I.

GENERAL OBSERVATION ON THE CONSTRUCTION OF AN ELECTRICAL APPARATUS.

IMPROVEMENTS in electrical machines have, as might well be expected, kept pace with improvements in the ſcience of electricity. While nothing more than electrical attraction and repulſion were known, nothing that we ſhould now call an *electrical apparatus* was neceſſary. Every thing that was known might be exhibited by means of a piece of amber, ſealing-wax, or glaſs; which the philoſopher rubbed againſt his coat, and preſented to bits of paper, feathers, and other light bodies that came in his way, and coſt him nothing.

To give a greater degree of friction to electric ſubſtances Otto Guericke and Mr. Haukſbee contrived to whirl ſulphur and glaſs in a ſpherical form; but their limited knowledge of electricity did not ſuggeſt, or require the more complex ſtructure of a modern electrical machine: Mr. Haukſbee's contrivances, indeed, were excellent, and the apparatus for many of his experiments well adapted to the purpoſes for which they were intended.

WHEN no farther uſe could be made of globes, philoſophers had recourſe to the eaſier and cheaper apparatus of glaſs tubes, and ſticks of ſulphur or ſealing-wax; and the firſt conductors they made uſe of were nothing more than hempen cords ſupported by ſilken lines. To theſe, bars of metal were ſoon ſubſtituted. After that, recourſe was again had to the globe, as much more convenient to give an uniform ſupply of electric matter to theſe inſulated conductors; and, in due time, a rubber was uſed to ſupply the place of a human hand.

THE diſcovery of the Leyden phial occaſioned ſtill more additions to our electrical apparatus; and the more modern diſcoveries of Dr. Franklin and others have likewiſe made proportionable additions highly requiſite. No philoſopher, for inſtance, can now be ſatisfied, if he be not able to ſupply a conductor from the clouds, as well as from the friction of his glaſs globes or tubes. But having already marked the progreſs of improvements in electrical machines, as well as in electrical ſcience, I ſhall content myſelf with this brief recapitulation, and proceed to deſcribe what experience (in many caſes dear bought) has taught me to think the beſt method of conſtructing machines, and to lay down the beſt rules for conducting electrical operations.

NOTWITHSTANDING globes or cylinders are now of the moſt extenſive uſe in electrical experiments, GLASS TUBES are, nevertheleſs, moſt convenient for ſeveral purpoſes, and no electrician ought to be without them. They ſhould be made as long as a perſon can well draw through his hand at one ſtroke, which is about three feet, or ſomething more; and as wide as he can conveniently graſp. The thickneſs of the metal is not material, perhaps the thinner they are, the better, if they will bear ſufficient friction; which, however, needs only to be very gentle, when the tube is in good order. It is moſt convenient to have the tube cloſed at one end: for, beſides that the electric matter is thereby retained beſt on its ſurface, the air may more eaſily be drawn out of it, or condenſed in it, by means of a braſs cap fitted to the open end. A tube thus furniſhed is requiſite for various experiments. [*a*, Pl. II.]

THE beſt rubber that has yet been found for a ſmooth glaſs tube is the rough ſide of black oiled ſilk, eſpecially when a little amalgam of mercury and any metal, is put upon it.

AN electrician ſhould be furniſhed with rough glaſs tubes, i. e. tubes with their poliſh taken off, as well as with ſmooth ones; but a cylinder of baked wood will do nearly as well. The beſt rubber for a rough glaſs tube, or a cylinder of baked wood, as well as for a ſtick of ſulphur or ſealing-wax, is ſoft new flannel; or rather ſkins, ſuch as hare ſkins, or cat ſkins, tanned with the hair on, being ſmoother, and having a more exquiſite poliſh.

ELECTRICIANS are not quite agreed whether the preference is, upon the whole, to be given to GLOBES or CYLINDERS. In favour of cylinders it is ſaid, that more of their ſurface may be touched by the rubber. On the other hand, in favour of globes, it is ſaid, that they can more eaſily be blown true, ſo as to preſs the rubber equally; they may alſo be made larger in diameter, and by this means, the axis (if they have any) may be farther from the excited ſurface: for when the *axes* are near the ſurface, the electric fire will ſeem to ſtrike them, ſo that they will ſometimes appear luminous in the dark, and if they be inſulated,

ſulated, the extremities of the axes will give ſparks; which is certainly a diminution of the electric fire at the conductor.

FOR this reaſon, I would adviſe, that all *axes* be avoided as much as poſſible, having found by experience, that they are in no caſe whatever neceſſary, the largeſt globes being whirled horizontally, with the greateſt eaſe, and in every reſpect to more advantage, with one neck than two. This method of fitting up globes alſo makes electrical machines much leſs complex, expenſive, and troubleſome.

LET every globe intended to be thus fitted up have its neck incloſed in a pretty deep braſs cap, ending in a dilated brim, of about half an inch broad, if the globe be a large one. To this neck let there be fitted a ſhort iron axis, and on that a PULLEY; and let a ſpace of about three quarters of an inch of the axis be left between the pulley and the cap. In this place the axis is to be ſupported by a ſtrong BRASS ARM [*c*, Pl. VII.] proceeding from the pillar into which the extremity of the axis is put, and in which it turns. This braſs arm may be made to receive globes of any ſize whatever, room being left in it for pullies of any ſize that may be wanted for them.

IN this manner globes may be fixed much more truly than they can with two necks, and they are mounted with much more eaſe, and leſs expence. The weight of large globes is no objection to this method. The largeſt need not to weigh above eight or ten pounds, and theſe have been found to turn with great eaſe in this manner. The rubber, if it be placed under the globe, will contribute to ſupport the weight of it.

LET there be a hole made in the braſs cap above mentioned, in order to preſerve a communication between the external air, and the air within the globe: for if the air within the globe be either rarer or denſer than that without, the excitation is found to be leſſened in proportion; and, judging from experience, nothing is to be apprehended from any moiſture which might be ſuppoſed to inſinuate itſelf into the globe by ſuch a communication. A difference, however, might be found in a damp ſituation.

It will be found convenient to have the axis project about an inch beyond the pillar in which it turns, [as at *d*, Pl. VII.] that a handle may be fitted to it, and that it may thereby be turned without a wheel, for the greater variety of experiments.

If an axis be uſed, let both the extremities of it be carefully turned in a lathe; otherwiſe it will not turn without a very diſagreeable rattling; and let the part within the globe be made round, and ſmooth, or covered with ſome electric ſubſtance, to prevent its taking off much of the electric virtue of the globe.

One of the pillars, in which theſe globes or cylinders with two axes are turned, ſhould be moveable; for then a globe or cylinder of any ſize may be uſed, and they ſhould be made high enough, and have holes at ſmall diſtances quite to the top, to take two globes upon occaſion, one above the other. [Plate VII.]

It has not yet been determined by electricians what kind of glaſs is the fitteſt for electrical purpoſes, but the beſt flint is commonly uſed. I have not made ſo many experiments, as I could wiſh, to aſcertain this circumſtance; as they are both very uncertain and expenſive; but I have ſome reaſon to think that common bottle metal is fitteſt for the purpoſe of excitation; at leaſt, the beſt globe I have yet ſeen is one that I have of that metal. Its virtue is certainly exceeding great, and I attribute it in part to the great hardneſs of the metal, and in part to its exquiſite poliſh. The blowing of any thing ſpherical in this metal, and eſpecially the making large globes ſmooth is very precarious; and they can hardly be made with two necks.

THE globe above mentioned is about ten inches in diameter, but nothing has been determined about the beſt ſize. I have uſed almoſt every ſize, from three inches to near eighteen in diameter, without knowing what advice to give. Perhaps *ceteris paribus*, twelve or thirteen inches may be, upon the whole, as convenient as any; but much larger, if they could be whirled with the ſame eaſe, would probably do better.

IF a perſon chuſe to have no aſſiſtant, but would turn the globe, and manage the apparatus himſelf, (which is, on many accounts, very deſirable) it will be moſt convenient to have the axis of the WHEEL level with the table at which he ſits. But if he chuſe rather to ſtand all the time he is making his experiments, it ſhould be raiſed proportionably higher. It will, perhaps, be moſt convenient to make the diameter of the wheel about eighteen inches; and the diameter of the pullies ſhould be ſuch as will give them, at leaſt, four or five revolutions for one of the wheel. For the globe ſhould generally revolve at leaſt four or five times in a ſecond, which is much ſwifter than it can be well turned without a wheel. But if a globe be very large, a wheel is leſs neceſſary.

THE wheel ſhould be made moveable with reſpect to the frame in which the globes are hung, or the frame ſhould be moveable with reſpect to the wheel, to ſuit the alterations which the weather will make in the length of the *ſtring*, particularly if it be made of hemp; but worſted makes an excellent ſtring, and is not ſo apt to alter with the weather, and a leathern ſtrap is perhaps better than either. If the diſtance between the wheel and the pulley cannot be altered, the operator muſt occaſionally moiſten his hempen ſtring, in order to make it tighter, which is, on many accounts, very inconvenient. Several grooves in the ſame wheel are very uſeful, and almoſt neceſſary, if more than one globe be uſed at the ſame time. If a flat leathern ſtrap be not uſed they ſhould be cut ſharp at the bottom; as ſhould alſo the grooves in the pulleys, that the ſtring may lay faſter hold of them, and that ſtrings of different ſizes may be uſed.

The beſt RUBBERS for globes or cylinders are made of red baſil ſkins, particularly the neck part of them, where the grain is more open, and the ſurface rather rough. That the rubber may preſs the globe equally, it ſhould be put upon a plate of metal bent to the ſhape of the globe, and be ſtuffed with any thing that is pretty ſoft. Bran is good; and if the ſtuffing be a conductor, as flax, it will be better than if it be a non-conductor, as hair, or wool. It ſhould reſt upon a ſpring, to favour any inequality there may be in the form of the globe or cylinder. The beſt poſition of the rubber, for a variety of purpoſes, is an horizontal one, but it ſhould be capable of being placed in every variety of horizontal poſition; and the ſpring which ſupports the rubber ſhould be made to preſs more or leſs at pleaſure. The rubber ſhould be made nearly as large one way as the other. If it be made very narrow, ſome parts of the globe will paſs it without a ſufficient friction. To remedy that inconvenience, the hand (if it be dry) may be held to the globe, juſt before the rubber, to add to its breadth; but that poſture is very inconvenient.

It is adviſable that there be no ſharp edges or angles about the rubber, for that would make the *inſulation* of it (which is a matter of great conſequence) ineffectual. By the inſulation of the rubber every electrical experiment may be performed with the twofold variety of poſitive and negative, and a conductor be made to give or take fire at pleaſure. This inſulation is beſt made by a plate of glaſs, five or ſix inches in diameter, [*g*. Pl. VII.] interpoſed between the metallic part of the rubber and the ſteel ſpring that ſupports it. When poſitive electricity is intended to be produced, a chain [*n*. Pl. VII.] muſt connect the rubber with the floor; but when negative electricity is wanted,

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the chain muſt be removed, and hung upon the common conductor, while another prime conductor muſt be connected with the rubber; which will therefore be electrified negatively.

THE beſt method of collecting the electric fire from the globe ſeems to be by three or four pointed wires, [*m*. Pl. VII.] two or three inches long, hanging lightly upon the globe; and neither ſo light as to be thrown off the globe by electrical repulſion (which would occaſion a loſs of the electric matter) nor ſo heavy as to prevent their ſeparating to a proper diſtance, and being drawn backwards or forwards, as the moſt effectual diſcharge of the fire, accumulated on different parts of the globe, may require. For this purpoſe they are beſt ſuſpended on an open metallic ring. Needles with fine points do admirably well.

IT is requiſite, for a variety of uſes, that the PRIME CONDUCTOR be fixed very ſteady. It ought not, therefore, to hang in ſilken ſtrings, but have a ſolid ſupport. In a dry ſituation baked wood anſwers very well; but a hollow pillar of glaſs lined with ſealing-wax is better, as it doth not require ſo much attention.

FOR common purpoſes a ſmall conductor is moſt convenient, but where a ſtrong ſpark is wanted, it is proper to have a large conductor at hand, which may be occaſionally placed in contact with the ſmaller, and be removed from it at pleaſure. But whatever be the ſize of a prime conductor, the extremity of it, or that part which is moſt remote from the globe, ſhould be made much larger and rounder than the reſt: [*k*. Pl. VII.] for the effort of the electric matter to fly off is always the greateſt at the greateſt diſtance from the globe. But for the ſame reaſon, if a *long* ſpark be wanted, the large conductor ſhould terminate in a ſmaller knob, or an obtuſe edge, at which the ſparks ſhould be ſolicited. Experience only can inſtruct a perſon what ſize of a knob, or what thickneſs of an edge is the beſt for the purpoſe. In this reſpect, the effects are often very different in the ſame apparatus, when the difference in the circumſtances is imperceptible.

As the electrician will have frequent occaſion to inſulate various bodies, I would adviſe that he make all the ſtands and ſtools which he uſes for that purpoſe of glaſs tubes lined with ſealing-wax, though in a dry ſituation; baked wood, eſpecially when covered with a ſlight varniſh, will do very well.

The electrician, having thus conſtructed his machine, will want METALLIC RODS, [*s.* Pl. II.] to take ſparks from his conductor for various uſes. Theſe ſhould have knobs, larger or ſmaller in proportion to the curvature of the conductor. If the knob be too ſmall, it will not diſcharge the conductor at once, but by degrees, and with a leſs ſenſible effect; whereas the ſpark between broad ſurfaces is thick and ſtrong.

The more formidable part of an electrical apparatus conſiſts in the COATED GLASS, that is uſed for the Leyden experiment. The form of the plate is immaterial with reſpect to the ſhock; and, for different experiments, both plates of glaſs, and jars, of various forms and ſizes, muſt be uſed. For common uſes, the moſt commodious form is that of a jar, as wide as a perſon can conveniently hold by graſping, and as tall as it will ſtand without any danger of falling; perhaps about three inches and a half in diameter, and eight inches in height. The mouth ſhould be pretty open, that it may be the more conveniently coated on the inſide, as well as the outſide, with tinfoil: but it will generally be moſt convenient to have the mouth narrower than the belly; for then it may be more eaſily kept clean and dry, and the cork, when one is wanted, will be eaſier to manage. But no electrician would chuſe to be without a great number of jars of various ſizes and forms. A conſiderable variety may be ſeen in plate II. fig, *c*, *d*, *e*, *f*, *g*, *h*, *i*, *j*, *k*. The form of a coated plate of glaſs is repreſented at *b*, in the ſame plate.

THE method of coating is much preferable to that of putting water or braſs ſhavings into the jars, which both makes them very heavy, and likewiſe incapable of being inverted, which is requiſite in many experiments. Braſs duſt, however, or leaden ſhot is very convenient for ſmall phials. Theſe ſerve very well where it is neceſſary to remove the coating as ſoon as the jar is charged, but, for this purpoſe, quickſilver will generally anſwer the beſt. The tinfoil may be put on either with paſte, gum water, or bees wax. To coat the inſides of veſſels, which have narrow mouths, moiſten the inſide with gum water, and then pour ſome braſs duſt upon it. Enough will ſtick on to make an exceedingly good coating; and if nothing very hard rub againſt it, it will not eaſily come off. This braſs duſt, which is extremely uſeful in a great variety of electrical experiments, may be had at the pin-makers.

IN the conſtruction of an ELECTRICAL BATTERY I would not, in general, recommend very large jars. A number of ſmaller are preferable on ſeveral accounts. If one of theſe break by an exploſion, or be cracked by any accident, the loſs is leſs conſiderable; beſides, by means of narrow jars, a greater force (that is a greater quantity of coated ſurface) may be contained in leſs room; and, as narrow jars may be made thinner, they will be capable of being charged higher in proportion to their ſurface than large jars, which muſt neceſſarily be made thick. The largeſt jars that the glaſs-men can conveniently make are about ſeventeen inches in height; and they ſhould not be more than three in diameter, and of the ſame width throughout. Thus they may be eaſily coated both within and without, and a box of a moderate ſize will contain a prodigious force: for the jars being coated within two inches of the top, they will contain a ſquare foot of coated glaſs a piece.

THE firſt battery that I conſtructed for my own uſe, conſiſted of forty-one jars of this ſize; but a great number of them burſting by ſpontaneous diſcharges, I conſtructed another, which I much prefer to it, and of which a drawing is given Plate III. It conſiſts of ſixty-four jars, each ten inches long, and two inches and a half in diameter, coated within an inch and a half of the top. The coated part of each is half a ſquare foot; ſo that the whole battery contains thirty-two ſquare feet. The wire of each jar has a piece of very ſmall wire twiſted about the lower end of it, to touch the inſide coating in ſeveral places; and it is put through a pretty large piece of cork, within the jar, to prevent any part of it touching the ſide, which would tend to promote a ſpontaneous diſcharge. Each wire is turned round, ſo as to make a hole at the upper end; and through theſe holes a pretty thick braſs rod with knobs is put, one rod ſerving for one row of the jars.

THE communication between theſe rods is made by laying over them all a thick chain, which is not drawn in the plate, leſt the figure ſhould appear too confuſed. If I chuſe to uſe only part of the battery, I lay the chain over as many rods as I want rows of jars. The bottom of the box, in which all the jars ſtand, is covered with a plate of tin, and a *bent wire*, touching this plate, is put through the box, and appears on the outſide, as in the plate. To this wire is faſtened whatever is intended to communicate with the outſide of the battery, as the piece of ſmall wire in the figure, and the diſcharge is made by bringing the braſs knob to any of the knobs of the battery.

THIS is the battery which I have generally uſed in the experiments related in the laſt part of this work; though, when I have wanted a very great force, I have joined both the batteries, and even ſeveral large jars to them. And it will perhaps be allowed to be ſome evidence of the goodneſs of this conſtruction,

tion, that after uſing it ſo much, I ſee no no reaſon to wiſh the leaſt alteration in any part of it. Were I to conſtruct another battery, I ſhould take jars of the very ſame ſize, and diſpoſe of them in the very ſame manner.

To diſcover the kind and degree of electricity, many forms of ELECTROMETERS have been thought of, as the reader may have perceived in the courſe of the hiſtory; but this buſineſs is ſtill imperfect. Mr. Canton's balls are of excellent uſe both to diſcover ſmall degrees of electricity, to obſerve the changes of it from poſitive to negative, and *vice verſa*; and to eſtimate the force of a ſhock before the diſcharge, ſo that the operator ſhall always be able to tell, very nearly, how high he has charged his jars, and what the exploſion will be whenever he chuſes to make it.

MR. CANTON'S BALLS (repreſented on a glaſs ſtanding on the ſtool [*c*. Plate II.] are only two pieces of cork, or pith of elder, nicely turned in a lathe, to about the ſize of a ſmall pea, and ſuſpended on fine linen threads. It is convenient to have theſe in ſmall boxes for the pocket; the box being the full length of the ſtrings, that they may lie there without being bent.

MR. KINNERSLEY'S electrometer, deſcribed p 204, is uſeful to aſcertain how great ſhocks have been, and for many curious experiments in electricity. A drawing is given of it [*r*. Plate II.] but the glaſs tube is repreſented as much ſhorter than it was made by Mr. Kinnerſley. I think it in general more convenient; as the bore of the ſmall tube may eaſily be proportioned to it. But if a perſon get one long tube, of the ſame ſize throughout, it may be cut into different lengths, and the ſame braſs caps will fit any part of it.

AT the top of the ſtand of baked wood which ſupports Mr. Kinnerſley's electrometer, I have fixed another, contrived by Mr. Lane, to give a number of ſhocks, all of preciſely the ſame degree of ſtrength. It conſiſts of a braſs rod furniſhed with a knob, which, by means of a fine ſcrew, may be fixed at any diſtance from the prime conductor, or any other fixed body communicating with the inſide of a jar or battery. In conſequence of this, the jar or battery, with which it is connected, can be charged no higher than the diſtance at which thoſe fixed bodies are placed will permit; for at that height of the charge, the exploſion will always be made between them. See Mr. Lane's more particular deſcription of this uſeful inſtrument *.

To the account of theſe articles of an electrical apparatus, which muſt be uſed within doors, it will not be wholly inſignificant to add, that a ſtrong firm table is highly requiſite. For if the TABLE on which the apparatus is diſpoſed be apt to ſhake, a great number of experiments cannot be performed to advantage.

In order to repeat the noble experiment of the ſameneſs of the electric fluid with the matter of lightning, and to make farther obſervations on the electricity of the atmoſphere, the electrician muſt be provided with a MACHINE FOR DRAWING ELECTRICITY FROM THE CLOUDS. For the beſt conſtruction of ſuch a machine, take the following directions. On the top of any building (which will be the more convenient if it ſtand upon an eminence) erect a pole [*a*, fig. 2. Pl. I.] as tall as a man can well manage, having on the top of it a ſolid piece of glaſs or baked wood, a foot in length. Let this be covered with a tin or copper veſſel [*b*] in the form of a funnel, to prevent its ever being wetted. Above this let there riſe a long ſlender rod [*c*] terminating in a pointed wire, and having a ſmall wire twiſted round its whole length, to conduct the electricity the better to the funnel. From the funnel make a wire [*d*] deſcend along the building, about a foot diſtance from it, and be conducted through an open ſaſh, into any room which ſhall be moſt convenient for managing the experiments. In this room let a proper conductor be inſulated, and connected with the wire coming in at the window. This wire and conductor, being completely inſulated, will be electrified whenever there is a conſiderable quantity of electricity in the air; and notice will be given when it is properly charged, either by Mr. Canton's balls, hung to it, or by ſuch a ſet of bells as will be deſcribed hereafter.

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* Phil. Tranſ. Vol. lvii. p. 451.

To make theſe experiments in perfect ſafety, the electrified wire ſhould be brought within a few inches of a conducting rod, that ſerves to guard the houſe, that the redundant electricity may off paſs that way, without ſtriking any perſon that may happen to ſtand near it. The conductor to guard the houſe ſhould conſiſt of one rod, between one fourth and one half of an inch thick, if it be of iron, but ſmaller if it be braſs or copper, terminating upwards in a ſharp point, about four or five feet above the higheſt part of the building; and below it ſhould, if poſſible, be continued to ſome well or running water. Otherwiſe it ſhould be ſunk ſeveral feet into the ground, at the diſtance of ſome yards from the building. It is of no conſequence whether this conducting rod be faſtened on the inſide, or outſide of the houſe, or how many bendings are made in it.

If the electrician be deſirous of making experiments upon the electricity of the atmoſphere to greater exactneſs, he muſt raiſe a kite, by means of a ſtring in which a ſmall wire is twiſted. The extremity of this line muſt be ſilk, and the wire muſt terminate in ſome metallic conductor, of ſuch a form as ſhall be thought moſt convenient. Mr. Romas's experiment will perhaps convince my reader, that it may be dangerous to raiſe this kite at the approach of a thunder ſtorm; and upon this occaſion the common apparatus above deſcribed for drawing electricity

tricity from the clouds will, probably, anſwer this purpoſe well enough.

But, with the following apparatus, I ſhould apprehend no great danger in any thunder ſtorm. Let the ſtring of the kite [*a* fig. 3. Pl. I.] be wound upon a reel [*b*] going through a ſlit in a flat board, faſtened at the top of it; by which more or leſs of the ſtring may be let out at pleaſure. Let the reel be fixed to the top of a tin or copper funnel [*c*] ſuch as was deſcribed above; and from the funnel let a metallic rod [*d*] with a large knob be projected, to ſerve for a conductor. This funnel and reel muſt be ſupported by a ſtaff [*e*] the upper end of which, at leaſt, muſt be well baked; and the lower end may be made ſharp, to thruſt into the ground, when the kite is well raiſed.

The ſafety of this apparatus depends upon the chain [*f*] faſtened to the ſtaff, by a hook a little below the funnel, and dragging on the ground: for the redundant lightning will ſtrike from the funnel to the chain, and ſo be conducted as far as any one chuſes, without touching the perſon who holds the ſtaff.

Sparks may be taken from the conductor belonging to this apparatus with all ſafety, by means of a ſmall rod of baked wood [*a* fig. 4.] furniſhed with a ſmall funnel [*b*] and a braſs rod [*c*] and a chain connected with it: for the lightning which ſtrikes the rod, will paſs by the funnel and the chain, without touching the perſon who holds the rod.

Mr. Hartman, in the conſtruction of his apparatus for obſerving the electricity of the atmoſphere with ſafety, makes uſe of long ſilken ſtrings to ſupport his metallic rod. Theſe, therefore, require a large ſhed, faſtened to the rod above them, to keep them dry. And, leſt the rain that falls upon this ſhed ſhould carry off all the electricity, he makes a channel all round, which receives the rain; and thence he conveys it, under the ſhed, into an inſulated receptacle *. But I cannot help thinking this complex apparatus unneceſſary, eſpecially if a ſolid ſtick of glaſs and a ſmall cover be uſed, inſtead of the ſilken ſtrings and large ſhed.

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* Anmerkungen, &c. p. 38.

SECTION II.

A DESCRIPTION OF SOME PARTICULAR ELECTRICAL MACHINES, WITH OBSERVATIONS ON THEIR PRINCIPAL ADVANTAGES AND DEFECTS.

AFTER this general account of the conſtruction of electrical machines, and the principal parts of an electrical apparatus, my reader may perhaps expect a more particular account of ſome of the principal varieties with which they are uſually made. And though it may be preſumed, from what has been advanced upon that head, that any perſon might judge for himſelf, I ſhall endeavour to gratify thoſe who are willing to provide themſelves with an electrical machine, by giving drawings and deſcriptions of ſome of the beſt conſtructions that have fallen under my notice, obſerving what I apprehend to be their ſeveral advantages and defects.

I SHALL begin with Mr. Haukſbee's machine [Plate IV. fig. 1.] which is an excellent conſtruction conſidering the ſtate of electricity in his time. The drawings annexed will render a very particular deſcription of this, or the other machine, unneceſſary. This has no rubber, no prime conductor, or field for making experiments; for no ſuch things were wanted in his time: but it may be eaſily accommodated with them all. A conductor may hang from the cieling, a rubber may be ſupported by a ſpring fixed ſixed under the globe, and a table placed near the machine, may receive the apparatus neceſſary for making experiments. The inconveniencies of this conſtruction are, that the operator cannot well turn the wheel himſelf. A ſervant is therefore neceſſary, who muſt ſit to his work. The machine admits only of one globe, or cylinder, which muſt have two necks; though it admits of a conſiderable variety of ſuch, and it is by no means portable.

THE Abbé Nollet's machine [Plate IV. fig. 2.] reſembles the greateſt number of the electrical machines that were uſed about the time that the Leyden phial was diſcovered. Theſe were the machines, heavy and unweildy as they ſeem, that were generally carried from place to place, when electrical experiments made a gainful buſineſs, and would bear the expence of the conveyance.

IN thoſe early times, electricians had no idea, that it was poſſible to make the globe revolve too ſwiftly. They, therefore, made their wheels exceedingly large, and the frame of the machine proportionably ſtrong. The globe was generally rubbed by the hand, the conductor was a bar of iron, or generally a gun barrel, ſuſpended in ſilken lines from the top of the room, and the apparatus was diſpoſed on an adjoining table.

THESE machines are now univerſally laid aſide, being more ſit for a large laboratory than a private ſtudy. Beſides, they neceſſarily require an aſſiſtant, and do not admit of half the variety in the diſpoſition of the principal parts of the conſtruction, which the variety of experiments now demands.

ABOUT the time that Mr. Boze's beatification was talked of, electricians were very deſirous of exciting a very great power of electricity; though, having no method of accumulating, or preſerving it, it was diſperſed as ſoon as raiſed. The machine repreſented in Plate V. fig. 1. was a contrivance of Dr. Watſon's, to whirl four large globes at a time, and unite the power of them all.

I cannot help regretting that no ſuch machines as theſe are conſtructed at this day, when, by means of electrical batteries, ſo great a power might be preſerved, and employed to the greateſt purpoſes. I wiſh the Doctor would refit the machine here deſcribed, if it be yet in being, and conſtruct a battery proportioned to it. But I ſhould rejoice more to ſee a machine moving by wind or water, turning twenty or thirty globes, and charging electrical batteries adequate to them. I make no doubt but that a full charge of two or three thouſand ſquare feet of coated glaſs would give a ſhock as great as a ſingle common flaſh of lightning. They are not philoſophers who will ſay, that nothing could be gained, and no new diſcoveries made by ſuch a power.

Plate V. fig. 2. exhibits a machine which Mr. Wilſon conſtructed, about the time above mentioned. It is much more commodious than any that had been contrived before, as all the parts are brought within a moderate compaſs; ſo that the ſame perſon may turn the wheel, and conduct the experiments.

Its inconveniences are, that it admits but little variety of globes or cylinders, and both theſe and the rubber are not ſufficiently diſtant from other bodies. The rubber is not inſulated, and the conductor is unſteady. This machine has a frame ſtanding upon the ground, but the general conſtruction may be preſerved, and the machine be made to ſcrew to a table. Some I have ſeen which, by this means, were made very portable; and a box was contrived in the inſide, to contain the apparatus.

Of the more modern conſtructions (of which there is an endleſs variety) the more elegant are thoſe in which the globe is turned by tooth and pinion. This reduces the wheel work, contained in the box [*a* Pl. VI. fig. 1] to an exceedingly ſmall compaſs, and gives the workmen an opportunity of making the machine all in braſs, very elegant, and portable. But I object to them, as liable to accidents, which electricians in general cannot eaſily repair; and I would wiſh philoſophers to be as independent as poſſible of all workmen. The conductor belonging to machines of this conſtruction is generally hung in ſilk, ſupported either by wooden pillars in a frame, as in the figure annexed, or by two braſs arms extending from the machine.

THESE machines are certainly very commodious for ſcrewing to a table. They require no aſſiſtant, and they admit of the experiments being made in a ſitting poſture; which is a great recommendation of a machine, to thoſe perſons who chuſe to do things with little trouble, and who are fond of a ſtudious ſedentary life. This conſtruction admits of very little variety in the ſize or number of globes, and hardly of a glaſs veſſel of any other form. But the greateſt inconvenience attending it, is the upright poſition of the globe and rubber, whereby every thing put upon it is apt to ſlip down; and the rubber is not inſulated.

IN the machine repreſented in Plate VI. fig. 2. and which was invented by Mr. Read, mathematical inſtrument maker at the quadrant in Knightſbridge, Hyde-park, a cylinder ſtands perpendicular to the horizon, ſupported by a braſs bow, which receives the upper end of the axis; and motion is given to it by means of a pulley at the lower end of the axis, and a wheel which lies parallel to the table. The conductor [*a*] is furniſhed with points to collect the fire, and it is ſcrewed to the wire of a coated jar [*b*], ſtanding in a ſocket, between the cylinder and the wheel. One machine of this kind I have ſeen, in which the cylinder and the wheel were not ſeparated by the conductor.

THIS conſtruction is peculiarly uſeful to phyſicians and apothecaries; and, with Mr. Lane's electrometer [*c*] annexed to it (the figure of which he has given me leave to inſert in the drawing annexed, taken from his own machine) as many ſhocks as are requiſite may be given, of preciſely the ſame, and any degree of force, without any change of poſture, either in the patient or the operator, who has nothing to do but turn the wheel, without ſo much as touching any other part of the apparatus.

When this machine is uſed for ſimple electrification, and other purpoſes where the ſhock is not required; the coated jar muſt be taken away, and another jar, without any coating, put in its place. By this means the conductor is fixed, which is a very great advantage, and which few machines are poſſeſſed of. But theſe machines, beſides that they admit no variety of globes or cylinders, and no inſulation of the rubber, require a motion of the arm, which I ſhould think not quite eaſy.

The ingenious Dr. Ingenhouſz of Vienna, and alſo Mr. Ramſden, mathematical inſtrument maker in the Haymarket, each independent of the other, conſtructed a machine in which friction is not given to any kind of hollow glaſs veſſel whatever; but to a *circular plate of glaſs*, generally about nine inches in diameter. This plate turns vertically, and rubs againſt four cuſhions, each an inch and a half long, placed at the oppoſite ends of the vertical diameter. The conductor is a braſs tube, has two horizontal branches coming from it, reaching within about half an inch of the extremity of the glaſs, ſo that each branch takes off the electricity excited by two of the cuſhions.

This conſtruction is original and ingenious, but the cuſhions cannot eaſily be inſulated, and a plate of glaſs is much more liable to injuries than a globe, or even a cylinder.

Mr. Wesley's people, I believe, generally uſe a machine in which two cylinders are turned by the ſame wheel: but one that I ſaw, in the poſſeſſion of a very intelligent perſon of that perſuaſion, had the cylinders and rubbers ſo confined in a cheſt, that, though it might do very well for medical uſes, it was very ill adapted to the purpoſes of philoſophy.

But the machine which I would adviſe a philoſopher to conſtruct for his own uſe, is that of which a drawing is given, Plate VII. This conſtruction is the reſult of my beſt attention to this ſubject. I have uſed it above ſix months (how much I leave the reader to imagine) without ſeeing the leaſt reaſon to make any alteration of conſequence in it; and believe it to have almoſt all the advantages, which an electrical machine deſigned for the cloſet can have. The reader will, therefore, allow me to be a little longer in the deſcription of it than I have been of the reſt.

The frame conſiſts of two ſtrong boards of mahogany [*a a*] of the ſame length, parallel to one another, about four inches aſunder; and the lower is an inch on each ſide broader than the upper. In the upper board is a *groove*, reaching almoſt its whole length. One of the *pillars* [*b*], which are of baked wood, is immoveable, being let through the upper board, and firmly fixed in the lower, while the other pillar ſlides in the groove above mentioned, in order to receive globes or cylinders of different ſizes; but it is only wanted when an axis is uſed. Both the pillars are perforated with *holes* at equal diſtances, from the top to the bottom; by means of which globes may be mounted higher or lower according to their ſize; and they are tall, to admit the uſe of two or more globes at a time, one above the other. Four of a moderate ſize may be uſed, if two be fixed on one axis: and the wheel has ſeveral grooves for that purpoſe.

If a globe with only one neck be uſed, as in the plate, a brass arm with an open ſocket [*c*] is neceſſary to ſupport the axis beyond the pulley; and this part is alſo contrived to be put higher or lower, together with the braſs ſocket in which the axis turns. The axis [*d*] is made to come quite through the pillar, that it may be turned by another handle, without the wheel, if the operator chuſes. The frame, being ſcrewed to the table, may be placed nearer to, or farther from the wheel, as the length of the ſtring requires, in different ſtates of the weather. The WHEEL is fixed in a frame by itſelf [*e*], by which it may have any ſituation with reſpect to the pulley, and be turned to one ſide, ſo as to prevent the ſtring from cutting itſelf. The hinder part of this frame is ſupported by a foot of its own.

THE RUBBER [*f*] conſiſts of a hollow piece of copper, filled with horſe hair, and covered with a baſil ſkin. It is ſupported by a ſocket, which receives the cylindrical axis of *a round plate of glaſs* [*g*], the oppoſite part of which is inſerted into the ſocket of a bent ſteel ſpring [*h*]. Theſe parts are eaſily ſeparated, ſo that the rubber, or the plate of glaſs that ſerves to inſulate it, may be changed at pleaſure. The SPRING admits of a twofold alteration of poſition. It may be either ſlipped along the groove, or moved in the contrary direction, ſo as to give it every deſirable poſition with reſpect to the globe or cylinder; and it is, beſides, furniſhed with a *ſcrew* [*i*], which makes it preſs harder or lighter, as the operator chuſes.

THE PRIME CONDUCTOR [*k*] is a hollow veſſel of poliſhed copper, in the form of a pear, ſupported by a pillar, and a firm baſis of baked wood, and it receives its fire by means of a *long arched wire, or rod of very ſoft braſs* [*l*], eaſily bent into any ſhape, and raiſed higher or lower, as the globe requires; and it is terminated by an *open ring*, in which are hung *ſome ſharp pointed wires* [*m*] playing lightly on the globe when it is in motion. The body of the conductor is furniſhed with *holes*, for the inſertion of metallic rods, to convey the fire wherever it is wanted, and for many other purpoſes convenient in a courſe of electrical experiments. The conductor is, by this means, ſteady, and yet may be eaſily put into any ſituation. It collects the fire perfectly well, and (what is of the greateſt conſequence, though but little attended to) retains it equally every where.

When poſitive electricity is wanted, a wire, or chain, as is repreſented in the plate [*n*] connects the rubber with the table or the floor. When negative electricity is wanted, that wire is connected with another conductor ſuch as that repreſented [*t*, Pl. II.] while the conductor in Plate VII. is connected by another wire or chain, with the table. If the rubber be made tolerably free from points, the negative power will be as ſtrong as the poſitive. In this machine I do not know which is the ſtronger of the two.

In ſhort, the capital advantages of this machine are, that glaſs veſſels, or any other electric body, of any ſize or form, may be uſed, with one neck, or two necks at pleaſure; and even ſeveral of them at the ſame time, if required. All the eſſential parts of the machine, the *globe*, the *frame*, the *wheel*, the *rubber*, and *conductor*, are quite ſeparate; and the poſition of them to one another may be varied in every manner poſſible. The rubber has a complete inſulation, by which means the operator may command either the negative or the poſitive power, and may change them in an inſtant. The conductor is ſteady, and eaſily enlarged, by rods inſerted into the holes, with which it is furniſhed, or by the conjunction of other conductors, in order to give larger ſparks, &c. The wheel may be uſed or not at pleaſure; ſo that the operator may either ſit, or ſtand to his work, as he pleaſes; and he may, with the utmoſt eaſe, both manage the wheel and his apparatus.

The machine repreſented in [Pl. VIII.] is conſtructed on the ſame general principles with the laſt. It is inferior to it in one reſpect, that it admits only of globes or cylinders with one neck, but theſe are far preferable to any other; and it is much more commodious for uſe, as it doth not require any ſtrong table like the other. It conſiſts of a pillar of mahogany [*a*] ſtanding upright on three feet. This pillar divides in two places, to receive a wheel [*b*] in the lower part of it, and in the upper part a pulley [*c*] which is turned by a leathern ſtrap [*d*] tightened by means of a ſmall buckle. In the center of the pulley is a ſtrong iron ſpindle, turning in two firm braſs ſockets, faſtened to each ſide of the pillar. In one of theſe ſockets the extremity of the ſpindle turns upon a center, by means of a piece of iron [*e*] ſcrewed into it, while the other is held tight by a braſs claſp, which may be made to hold it cloſer, or more looſely, at pleaſure, by means of a ſcrew [*f*]. The iron ſpindle terminates in a male ſcrew, anſwering to a female ſcrew in the braſs cap of the globe [*g*]; and by this means any globe may be taken out, and another put into the machine with very little trouble, if theſe parts be always made to the ſame pattern.

The rubber [*h*] is ſeparated from the ſpring [*i*] by a plate of glaſs [*j*], which effectually inſulates it; but the chain [*k*] connects them together when poſitive electricity is wanted, as in the uſual method of electrifying. The ſpring may be made to preſs more or leſs, by means of a ſcrew [*l*]; and it may be raiſed higher or lower, to ſuit globes of different ſizes, by means of a contrivance which is not repreſented in the plate.

The prime conductor *m*, *n*, *o*, is the ſame as in Pl. VII. From the ſame board which ſupports it, ariſes another pillar, at the top of which is Mr. Lane's electrometer; the knob of which [*p*] may be placed as near to the knob oppoſite to it on the prime conductor [*q*] as is deſired, by means of the graduated part [*r*]. But the whole of this may be taken away when it is not wanted.

When negative electricity is deſired, the chain [*k*] muſt be removed from the rubber, and hung upon the prime conductor, ſo as to connect it with the table; and a ſhort braſs rod, with a knob at the end of it, muſt be ſcrewed into a ſmall ſocket, which will be found in the rubber above the plate of glaſs. This braſs rod will then ſerve for a negative prime conductor; for, in this ſituation, when the wheel of the machine is turned, this rod, being inſulated (together with the rubber, through which all the electric fire paſſes to the globe) will receive ſparks from whatever is preſented to it, and therefore electrify negatively.

As it requires ſome dexterity and experience to turn the machine, ſtanding on three feet only, without ſhaking it; ſmall plates of braſs, upon which the edges of heavy weights, made of lead or iron, may be placed, are faſtened to two of them; but a large board may be firmly ſcrewed under all the feet, or various other methods may be uſed, whereby the pillar, which ſupports the machine, may ſtand as firm as a perſon chuſes.

PART VI.

PRACTICAL MAXIMS FOR THE USE OF YOUNG ELECTRICIANS.

AS the chapter I am now entering upon is profeſſedly deſigned for the uſe of young electricians, it is hoped that the proficient will excuſe my inſerting a few plain and trite maxims; which, though they be ſuperfluous with reſpect to him, may not be ſo to all my readers. The greateſt electricians (who are generally thoſe who have had the feweſt inſtructions) may remember the time when the knowledge of a rule or maxim, which they would perhaps ſmile to ſee in a book, would have ſaved them a great deal of trouble and expence; and it is hoped they will not envy others acquiring wiſdom cheaper than they did. In a general treatiſe, every man has an equal right to expect to find what he wants; and it is for the intereſt of the ſcience in general, that every thing be made as eaſy and inviting as poſſible to beginners. It is this circumſtance only that can increaſe the number of electricians, and it is from the increaſe of this number that we may moſt reaſonably expect improvements in the ſcience.

WHEN the air is dry, particularly when the weather is froſty, and when the wind is North, or Eaſt, there is hardly any electrical machine but will work very well. If the air be damp, let the room in which the machine is uſed be well aired with a fire, and let the globe and every thing about it be made very dry, and it may be made to work almoſt as well as in the beſt ſtate of the air.

WHEN a tube is uſed, the hand ſhould be kept two or three inches below the upper part of the rubber; otherwiſe the electricity will diſcharge itſelf upon the hand, and nothing will remain upon the tube for electrical purpoſes.

A LITTLE bees wax drawn over the ſurface of a tube will greatly increaſe its power. When the tube is in very good order, and highly excited, it will, at every ſtroke, throw off many pencils of rays from its ſurface, without the approach of any conductor, except what may float in the common atmoſphere.

IT has been the cuſtom of many electricians to line their globes with ſealing wax, or ſome other electric ſubſtance, in order to make them act with more eaſe and vigour. Mr. C. L. Epinaſſe gives the following receipt for an electric compoſition for this purpoſe. Take four pounds of Venice turpentine, one pound of roſin, and one pound of bees wax. Boil theſe over a gentle fire, ſtirring them now and then, for four hours; at the end of which ſtir in one quarter of a pound of vermilion. Then a little of the mixture being taken out and left to cool, will be hard and brittle, a token that it is fit for uſe. Having well heated your globe or cylinder, pour the melted mixture into it, and turn it about, ſo as to ſpread it evenly over the inſide ſurface, to the thickneſs of a ſixpence, and let it cool very gradually*.

I MAKE no doubt but that this electric lining is uſeful in ſome caſes, eſpecially in keeping the inſide of a globe free from moiſture, which is more apt to adhere to glaſs than other electric ſubſtances. It will be ſeen that a lining of ſulphur was remarkably uſeful in the caſe of two large globes of my own, the hiſtory of which will be given in the laſt part of this work.

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* Phil. Tranſ. Vol. lvii. p. 186.

To increaſe the quantity of electric fire from a globe, moiſten the rubber a little from time to time; or rather moiſten the under ſide of a looſe piece of leather, which may occaſionally be put upon the rubber. But the moſt powerful exciter of electricity is a little amalgam, which may be made by rubbing together mercury and thin pieces of lead or tinfoil in the palm of the hand. If the rubber ſhould be placed perpendicular to the horizon, it will be neceſſary to uſe a little tallow to make it ſtick. With this excellent reſource, almoſt all ſtates of the weather are equal to an electrician.

A LITTE time after freſh amalgam has been put upon the globe, and often at other times, if there be any foulneſs upon the cuſhion; and ſometimes when there is none, there will be formed upon the globe ſmall black ſpots, of a hard rough ſubſtance, which grow continually larger, till a conſiderable quantity of that matter be accumulated upon the ſurface. This muſt be carefully picked off, or it will obſtruct the excitation, and in a great meaſure defeat the electrical operations.

WHEN the amalgam has been uſed for ſome time, there will be formed upon the rubber a thick incruſtation of the ſame kind of black ſubſtance which is apt to adhere to the globe. This incruſtation is a very great improvement of the rubber: For when once a conſiderable body of this matter is formed, and it is a little moiſtened, or ſcraped, as much fire will be produced, as if freſh amalgam were uſed: ſo that it ſeems almoſt to ſuperſede the farther uſe of the amalgam.

As the electric matter is only collected at the rubber, it is neceſſary that it have a communication with the common maſs of the earth, by means of good conductors. If, therefore, the table on which the machine ſtands, or the floor of the room in which it is uſed, be very dry, little or no fire will be got, be the machine ever ſo good. In this caſe it will be neceſſary to connect the rubber, by means of chains or wires, with the floor, or even the next water, if the neighbouring ground be dry. This Dr. Franklin informs me he was frequently obliged to do in Philadelphia.

When the electricity of a globe is very vigorous, the electric fire will ſeem to dart from the cuſhion towards the wire of the conductor. I have ſeen thoſe lucid rays (which are viſible in day-light) make the circuit of half the globe, and reach the wires: and they will frequently come in a conſiderable number, at the ſame time, from different parts of the cuſhion. The noiſe attending this beautiful phenomenon exactly reſembles the crackling of bay leaves in the fire. Frequently theſe lucid arches have *radiant points*, often four or five in different parts of the ſame arch. Theſe radiant points are intenſely bright, and appear very beautiful. It is peculiarly pleaſing to obſerve theſe circles of fire riſe from thoſe parts of the cuſhion where the amalgam or moiſture has been put, or which have been lately ſcraped. Single points on the rubber will then ſeem intenſely bright, and for a long time together will ſeem to pour out continual torrents of flame. If one part of the rubber be preſſed cloſer than another, the circles will iſſue in that place more frequently than in any other.

When the conductor is taken quite away, circles of fire will appear on both ſides the rubber, which will ſometimes meet, and completely encircle the globe. If a finger be brought within half an inch of the globe, in that ſtate, it is ſure to be ſtruck very ſmartly, and there will often be a complete arch of fire from it to the rubber, though it be almoſt quite round the globe.

THE ſmaller the conductor is made, the more fire may be collected from it: for there is leſs ſurface from which the fire may eſcape. But in charging a phial, if the wire be placed cloſe to the conductor, the difference will be inconſiderable, whether a ſmaller or larger conductor be uſed, till it begin to be charged pretty high; for, till that time, the conductor will not have acquired any conſiderable atmoſphere.

IF the conductor be made perfectly well, and the air be dry, there will never be any loſs of fire from any part of it. For when the whole ſurface has received as high a charge as the machine can give to it, it will, in all places alike, perfectly reſiſt all farther efforts to throw more upon it, and the circulation of the fluid by the rubber will be ſtopped, being balanced, as it were, by equal forces. Or if it loſe in all places alike, the diſſipation muſt be inviſible. This maxim almoſt admits of ocular demonſtration. For when the rubber is perfectly inſulated, and the conductor has an opportunity of diſcharging itſelf, the rubber will take ſparks from a wire placed near it very faſt; but when the conductor has little opportunity of emptying itſelf, it will take fewer of thoſe ſparks.

To form a juſt eſtimate of the electrical power of any machine, and to compare different machines in this reſpect, take two wires with knobs of any ſize, and fix one of them at the conductor, and the other at ſome certain diſtance from it, about an inch, or an inch and a half; and when the wheel is turned, count the number of ſparks that paſs between them during any given time. Fix the ſame wires to any other conductor, belonging to any other machine (but the ſame conductor would be more exact) and the difference between the number of ſtrokes in any given time will aſcertain the difference between the ſtrength of the two machines.

The larger the conductor is made, the ſtronger ſpark it will give: for the more extended the electrified ſurface is, the greater quantity of the electric atmoſphere it contains, and the more ſenſible will be its effects when it is all diſcharged at once. The conductor, however, may be made ſo large, that the neceſſary diſſipation of the electric matter from its ſurface into the air will be equal to the ſupply from the machine, which will conſtitute the MAXIMUM of the power of that machine, and will be different in different ſtates of the air.

A CERTAIN degree of friction is neceſſary to give a globe its greateſt power. A number of globes increaſes the power, but the increaſe of friction will make it more difficult for a man to excite their power. A few trials with a number of globes would enable any man to judge of the *maximum* of his ſtrength in exciting electricity. I ſhould imagine, from my own experience, that no perſon could excite much more electricity from any number of globes, than he could from one; ſuppoſing him to continue the operation an hour, or even only half an hour together.

WHEN a long conductor is uſed, the longeſt and the ſtrongeſt ſpark may be drawn from the extremity of it, or from that part which is the moſt remote from the globe.

VERY large and pungent ſparks are often drawn from any conductor along an electric ſubſtance. Thus if the conductor be ſupported by pillars of glaſs or of baked wood, theſe ſparks will be taken cloſe to the pillar.

IF the conductor bend inwards in any place, ſo as to make the ſurface concave, a peculiarly large, ſtrong, and undivided ſpark may be drawn from that place. Where the ſurface is convex, the ſpark is more apt to be divided and weakened.

IF a ſmooth cork ball be hung in a long ſilken ſtring, and electrified poſitively, it will always be repelled by poſitive, and attracted by negative electricity. But the ſtrongeſt repulſion will be changed into attraction at a certain diſtance.

IF two pith balls hung by linen threads, and diverging with poſitive electricity, be inſulated, though in connection with conductors of conſiderable length, the approach of a body electrified poſitively will firſt make them ſeparate, and then (if the electricity of the balls be ſmall, and that of the approaching body great) it will, at a certain diſtance, make them approach, and at length come into contact with it. Sometimes the divergence previous to the convergence is very ſlight, and, without great attention, is apt to be overlooked.

IF the balls have a free communication with the earth, for inſtance, if they be held in the hand of a perſon ſtanding on the ground, and (as in the former caſe) they be made to diverge with poſitive electricity, in conſequence of being held within the influence of a body electrified negatively, the approach of poſitive electricity will make them converge, and negative elecricity will make them diverge: the electric matter of the approaching body, in the former caſe, repelling that of the balls, and thereby, as it were, unelectrifying them; whereas, in the latter caſe, the negative electricity of an approaching body draws it more powerfully into the threads, and makes them diverge more. This method of judging is, therefore, excellently adapted to aſcertain the kind of electricity in the atmoſphere, or of a charged jar or battery, the balls being held in the hand of a perſon ſtanding on the earth or the floor.

To diſcover ſmaller degrees of electricity than the balls can ſhow to advantage, uſe a very fine thread, or two of them. If inſulation be neceſſary, faſten it to a ſtick of baked wood. But the moſt accurate meaſure of electricity I have yet hit upon, is a ſingle thread of ſilk as it comes from the worm. When the end of this has received a ſmall degree of electricity, it will

retain

retain it a conſiderable time, and the ſlighteſt electric force will give it motion. Before any experiments be made, let it be carefully obſerved how long, in any particular ſituation, it will retain the degree of electricity that is intended to be given to it; and let allowance be made for that in the courſe of the experiments. It will retain electricity much longer, if a ſmall piece of down from a feather be faſtened to it, but it will not acquire the virtue ſo ſoon. And it will be moſt eaſy to manage, if two or three threads of ſilk be uſed, and the piece of down be ſo adjuſted to them, that it ſhall but juſt prefer a perpendicular ſituation, and not abſolutely float in the air at random. This electrometer is not liable to the inaccuracies of thoſe that have a ſenſible weight: for as there is always a ſphere of attraction within a ſphere of repulſion, the weight of the electrified body will allow another to paſs the boundary of thoſe two ſpheres, without a ſenſible obſtruction; but the body I am deſcribing immediately retires, with all its ſpheres of attraction and repulſion about it.

THE force of the ELECTRIC SHOCK is in proportion to the quantity of ſurface coated, the thinneſs of the glaſs, and the power of the machine. That this laſt circumſtance ought to be taken into conſideration is evident: for different machines will charge the ſame jar very differently. With one machine, for inſtance, it may be made to diſcharge itſelf, when it cannot with another.

THE moſt effectual method of charging a jar is to connect the outſide, by means of wires, with the rubber, while the wire proceeding from the inſide is in contact with the conductor. In this manner the inſide of the jar will be ſupplied with the very ſame fire that left the outſide. In this caſe alſo the jar will receive as high a charge as it is capable of receiving, though the rubber be inſulated, and have no communication but with the outſide coating; ſo that, in the caſe of charging, there can be no occaſion for the directions given above, when the table, the floor of the room, or the ground are very dry.

THE greateſt quantity of fire that a jar will hold is not always the quantity it will contain when it is coated juſt ſo low as not to diſcharge itſelf. In this caſe, indeed, the part that is coated is charged as high as it can be, but then a conſiderable part of the ſurface is not charged at all, or very imperfectly. On the other hand, if the jar be coated very high, it may be made to diſcharge itſelf with as ſmall an exploſion as one chuſes. The exact *maximum* of the charge of any jar is not eaſy to aſcertain.

THE greateſt effort in a jar to make a diſcharge ſeems to be about half a minute, or a minute, after it is removed from the conductor, owing, perhaps, to non-electric duſt or moiſture attracted by and adhering to the glaſs, between the outſide and inſide coating; ſo that if there be any apprehenſion of its diſcharging itſelf, it is adviſeable to diſcharge it before it has ſtood charged at all.

WHEN a thin jar is diſcharged, it is adviſable not to do it by placing the diſcharging rod oppoſite to the thinneſt part. It will endanger the burſting of the jar in that place.

THE more perſons join hands to take a ſhock the weaker it is.

IF two jars, of the ſame thickneſs, be uſed together, the ſtronger of them will receive no higher a charge than the weaker. If one of them, for inſtance, be coated ſo high as that it will diſcharge itſelf, either with or without burſting, after a few turns of the wheel; the other will always be diſcharged along with it, though it was capable of being charged ever ſo high by itſelf. The method, therefore, of eſtimating the force of a number of jars, is to conſider each of them as capable of containing no more fire than the weakeſt in the company. It follows from hence, that if a ſingle jar in a large battery have the ſmalleſt crack in the coated part of it, not one of them is capable of being charged in conjunction with it.

In large batteries, it is adviſable to coat the jars pretty high, the diſſipation of the electric matter from ſo great a ſurface when the charge is high being very conſiderable. The battery might be made ſo large, as that after a very moderate charge, the machine would be able to throw no more fire in than was was exhaled, as we may ſay, from the ſurface. This would be the MAXIMUM OF THE POWER OF THAT MACHINE IN CHARGING.

In order to judge of the ſtrength of a charge (which, in large batteries, is a thing of conſiderable conſequence) preſent Mr. Canton's balls to the wires, from time to time. A compariſon of the degree of their divergence, compared with the actual exploſion, will ſoon enable the operator to tell how high his battery is charged, and what will be the force of the exploſion.

In comparing different exploſions by their power to melt wires, let it be obſerved, that, in wires of the ſame thickneſs, the forces that melt them will be in the proportion of the lengths; and in wires of the ſame length, in the proportion of the ſquares of their diameters.

Do not expect that the exploſion of a battery will pierce a number of leaves of paper in proportion to its force in other reſpects. That depends upon the height of the charge much more than the quantity of coated ſurface. I have known an exploſion which would have melted a pretty thick wire not able to pierce the cover of a book, which a ſmall common jar would have done with eaſe. If it had been pierced with the exploſion of the battery, the hole would have been larger in proportion.

LET no perſon imagine that, becauſe he can handle the wires of a large battery without feeling any thing, that therefore he may ſafely touch the outſide coating with one hand, while the other is upon them. I have more than once received ſhocks that I ſhould not like to receive again, when the wires ſhowed no ſign of a charge; even two days after the diſcharge, and when papers, books, my hat, and many other things had lain upon them the greateſt part of the time. If the box be tolerably dry, the reſiduum of the charge will not diſperſe very ſoon. I have known even the *reſiduum of a reſiduum* in my batteries to remain in them ſeveral days. For preſently after an exploſion, I ſeldom fail to diſcharge the reſiduum, which, in ſome caſes, is very conſiderable, for fear of a diſagreeable accident.

A SMALL ſhock paſſing through the body gives a ſenſation much more acute and pungent than a large one. I cannot boaſt, like Dr. Franklin, of being twice ſtruck ſenſeleſs by the electric ſhock; but I once, inadvertently, received the full charge of two jars, each containing three ſquare feet of coated glaſs. The ſtroke could not be called painful, but, though it paſſed through my arms and breaſt only, it ſeemed to affect every part of my body alike. The only inconvenience I felt from it was a laſſitude, which went off in about two hours.

MR. WILCKE was ſtruck down ſenſeleſs, by accidentally receiving, from his head to his feet, the charge of a large chymical receiver. He thought, that if he had received a ſhock five times as large, he ſhould never have written the account of it *.

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* Remarks on Franklin's Letters, p. 362.

PART VII.

A DESCRIPTION OF THE MOST ENTERTAINING EXPERIMENTS PERFORMED BY ELECTRICITY.

ELECTRICITY has one conſiderable advantage over moſt other branches of ſcience, as it both furniſhes matter of ſpeculation for philoſophers, and of entertainment for all perſons promiſcuouſly. Neither the air-pump, nor the orrery; neither experiments in hydroſtatics, optics, or magnetiſm; nor thoſe in all other branches of Natural Philoſophy ever brought together ſo many, or ſo great concourſes of people, as thoſe of electricity have done ſingly. Electrical experiments have, in almoſt every country in Europe, occaſionally furniſhed the means of ſubſiſtence to numbers of ingenious and induſtrious perſons, whoſe circumſtances have not been affluent, and who have had the addreſs to turn to their own advantage that paſſion for the marvellous, which they ſaw to be ſo ſtrong in all their fellow-creatures. A man need not deſire a greater income than the ſums which have been received in ſhillings, ſix-pences, three-pences, and two-pences, for exhibiting the Leyden experiment.

IF we only conſider what it is in objects that makes them capable of exciting that pleaſing aſtoniſhment, which has ſuch charms for all mankind, we ſhall not wonder at the eagerneſs with which perſons of both ſexes, and of every age and condition, run to ſee electrical experiments. Here we ſee the courſe of nature, to all appearance, intirely reverſed, in its moſt fundamental laws, and by cauſes ſeemingly the ſlighteſt imaginable. And not only are the greateſt effects produced by cauſes which ſeem to be inconſiderable, but by thoſe with which they ſeem to have no connection. Here, contrary to the principles of gravitation, we ſee bodies attracted, repelled, and held ſuſpended by others, which are ſeen to have acquired that power by nothing but a very ſlight friction; while another body, with the very ſame friction, reverſes all its effects. Here we ſee a piece of cold metal, or even water, or ice, emitting ſtrong ſparks of fire, ſo as to kindle many inflammable ſubſtances; and *in vacuo* its light is prodigiouſly diffuſed and copious, ſo as exactly to reſemble, what it really is, the lightning of heaven. Again, what can ſeem more miraculous than to find, that a common glaſs phial or jar, ſhould, after a little preparation (which, however, leaves no viſible effect, whereby it could be diſtinguiſhed from other phials or jars) be capable of giving a perſon ſuch a violent ſenſation, as nothing elſe in nature can give, and even of deſtroying animal life; and this ſhock attended with an exploſion like thunder, and a flaſh like that of lightning? Laſtly, what would the ancient philoſophers, what would Newton himſelf have ſaid, to ſee the preſent race of electricians imitating in miniature all the known effects of that tremendous power, nay diſarming the thunder of its power of doing miſchief, and, without any apprehenſion of danger to themſelves, drawing lightning from the clouds into a private room, and amuſing themſelves at their leiſure, by performing with it all the experiments that are exhibited by electrical machines.

So far are philoſophers from laughing to ſee the aſtoniſhment of the vulgar at theſe experiments, that they cannot help viewing them with equal, if not greater aſtoniſhment themſelves. Indeed, all the electricians of the preſent age can well remember the time, when, with reſpect to theſe things, they themſelves would have ranked among the ſame ignorant ſtaring vulgar.

BESIDES, ſo imperfectly are theſe ſtrange appearances underſtood, that philoſophers themſelves cannot be too well acquainted with them; and therefore ſhould not avoid frequent opportunities of ſeeing the ſame things, and viewing them in every light. It is poſſible that, in the moſt common appearances, ſome circumſtance or other, which had not been attended to, may ſtrike them; and that from thence light may be reflected upon many other electrical appearances.

WHETHER philoſophers may think this conſideration worth attending to or not, I ſhall, for the ſake of thoſe electricians who are young enough, and, as it may be thought, childiſh enough, to divert themſelves and their friends with electrical experiments, deſcribe a number of the moſt beautiful and ſurpriſing appearances in electricity; that the young operator may not be at a loſs what to exhibit when a company of gentlemen or ladies wait upon him, and that he may be able to perform the experiments to the moſt advantage, without diſappointing his friends, or fretting himſelf.

To make this buſineſs the eaſier to the young operator, I ſhall conſult his convenience in the order in which I ſhall relate the experiments, beginning with thoſe which only require ſimple electrification, then proceeding to thoſe in which the Leyden experiment is uſed, and concluding with thoſe in which recourſe muſt be had to other philoſophical inſtruments in conjunction with the electrical machine.

SECTION I.

ENTERTAINING EXPERIMENTS IN WHICH THE LEYDEN PHIAL IS NOT USED.

THE phenomena of electrical attraction are ſhown in as pleaſing a manner by the tube, as they can be by any methods that have been found out ſince the later improvements in electricity. It is really ſurpriſing to ſee a feather, or a piece of leaf gold firſt attracted by a glaſs tube excited by a ſlight friction, then repelled by it, and held ſuſpended in the air, or driven about the room wherever the operator pleaſes; and the ſurpriſe is increaſed by ſeeing the feather, which was repelled by the ſmooth glaſs tube, attracted by an excited rough tube, or a ſtick of ſealing wax, &c. and jumping from the one to the other, till the electricity of both be diſcharged. Nor is the obſervation of Otto Guericke the leaſt pleaſing circumſtance, viz. that in turning the tube round the feather, the ſame ſide of the feather is always preſented towards it.

BUT ſince electrical ſubſtances part with their electricity but ſlowly, the more rapid alternate attractions and repulſions are ſhown to the beſt advantage at the prime conductor. Thus preſent a number of ſeeds of any kind, grains of ſand, a quantity of braſs duſt, or other light ſubſtances in a metal diſh (or rather in a glaſs cylindrical veſſel ſtanding on a metal plate) to another plate hanging from the conductor [as at *n* and *o*, Pl. II.] and the light ſubſtances will be attracted and repelled with inconceivable rapidity, ſo as to exhibit a perfect ſhower, which, in the dark will be all luminous.

SUSPEND one plate of metal to the conductor, and place a metal ſtand, of the ſame ſize, at the diſtance of a few inches exactly under it, and upon the ſtand put the figures of men, animals, or whatever elſe ſhall be imagined, cut in paper or leaf gold, and pretty ſharply pointed at both extremities; and then, upon electrifying the upper plate, they will perform a dance, with amazing rapidity of motion, and to the great diverſion of the ſpectators.

IF a downy feather, or a piece of thiſtle down be uſed in this manner, it will be attracted and repelled with ſuch aſtoniſhing celerity, that both its form and motion will diſappear; all that is to be diſcerned being its colour only, which will uniformly fill the whole ſpace in which it vibrates *.

IF a piece of leaf gold be cut with a pretty large angle at one extremity, and a very acute one at the other, it will need no lower plate, but will hang by its larger angle at a ſmall diſtance from the conductor, and by the continual waving motion of its lower extremities, will have the appearance of ſomething animated, biting or nibling at the conductor. It is therefore called by Dr. Franklin the *golden fiſh*.

To the dancing figures above mentioned, it is very amuſing to add a ſet of ELECTRICAL BELLS. Theſe conſiſt of three ſmall bells, the two outermoſt of which are ſuſpended from the conductor by chains, and that in the middle by a ſilken ſtring, while a chain connects it with the floor; and two ſmall knobs of braſs, to ſerve inſtead of clappers, hang by ſilken ſtrings, one between each two bells. In conſequence of this diſpoſition, when the two outermoſt bells, communicating with the conductor, are electrified, they will attract the clappers, and be ſtruck by them. The clappers, being thus loaded with electricity, will be repelled, and fly to diſcharge themſelves upon the middle bell. After this, they will be again attracted by the outermoſt bells; and thus by ſtriking the bells alternately, a continual ringing may be kept up as long as the operator pleaſes. In the dark, a continual flaſhing of light will be ſeen between the clappers and the bells; and when the electrification is very ſtrong, theſe flaſhes of light will be ſo large, that they will be tranſmitted by the clapper from one bell to the other, without its ever coming into actual contact with either of them, and the ringing will, conſequently, ceaſe. When theſe two experiments of the bells and the figures are exhibited at the ſame time, they have the appearance of men or animals dancing to the muſic of the bells; which, if well conducted, may be very diverting.

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* Lovet, p. 28.

If a piece of burnt cork, about the bigneſs of a pea, cut into the form of a ſpider, with legs of linen thread, and a grain or two of lead put in it, to give it more weight, be ſuſpended by a fine ſilken thread, it will, like a clapper between the two bells, jump from an electrified to an unelectrified body and back again, or between two bodies poſſeſſed of different electricities, moving its legs as if it were alive, to the great ſurprize of perſons unacquainted with the conſtruction of it. This is an American invention, and is deſcribed by Dr. Franklin *.

Several very beautiful experiments, which depend on electrical repulſion, may be ſhown to great advantage by bundles of thread, or of hair, ſuſpended from the conductor, or preſented to it. They will ſuddenly ſtart up, and ſeparate upon being electrified, and inſtantly collapſe when the electricity is taken

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* Letters, p. 17.

off. If the operator can manage this experiment with any degree of dexterity, the hair will ſeem to the company to riſe and fall at the word of command.

IF a large plumy feather be fixed upright on an electrified ſtand, or held in the hand of a perſon electrified, it is very pleaſing to obſerve how it becomes turgid, its fibres extending themſelves in all directions from the rib; and how it ſhrinks, like the ſenſitive plant, when any unelectrified body touches it, when the point of a pin or needle is preſented to it, or when the prime conductor with which it is connected.

BUT the effects of electrical repulſion are ſhown in a more ſurpriſing manner by means of water iſſuing out of a capillary tube. If a veſſel of water be ſuſpended from the conductor, and a capillary ſyphon be put into it, the water will iſſue ſlowly, and in the form of large drops from the lower leg of the ſyphon; but, upon electrifying this little apparatus, inſtead of drops, there will be one continued ſtream of water; and if the electrification be ſtrong, a number of ſtreams, in the form of a cone, the apex of which will be at the extremity of the tube; and this beautiful ſhower will be luminous in the dark.

LASTLY, Mr. Rackſtrow's experiment (as it is generally called, but which Mr. Henley informs me was really invented by John Serocold, Eſq.) is a ſtriking inſtance of electrical attraction and repulſion, and, at the ſame time, exhibits a very pleaſing ſpectacle. Electrify a hoop of metal, ſuſpended from the prime conductor (or ſupported with ſmall pieces of ſealing wax, &c.) about half an inch above a plate of metal, and parallel to it. Then place a round glaſs bubble, blown very light, upon the plate, near the hoop, and it will be immediately attracted to it. In conſequence of this, the part of the bubble which touched the hoop will acquire ſome electric virtue, and be repelled; and, the electricity not being diffuſed over the whole ſurface of the glaſs, another part of the ſurface will be attracted, while the former goes to diſcharge its electricity upon the place. This will produce a revolution of the bubble quite round the hoop, as long as the electrification is continued; and it will be either way, juſt as it happens to ſet out, or as it is driven by the operator. If the room be darkened the glaſs ball will be beautifully illuminated. Two bubbles may be made to revolve about the ſame hoop, one on the inſide, and the other on the outſide; and either in the ſame, or contrary directions. If more hoops be uſed, a greater number of bubbles may be made to revolve, and thus a kind of *planetarium* or *orrery* might be conſtructed, and a ball hung over the center of all the hoops would ſerve to repreſent the ſun in the center of the ſyſtem. Or the hoops might be made elliptical, and the ſun be placed in one of the *foci*. N. B. A bell or any metallic veſſel inverted would ſerve inſtead of a ſingle hoop.

All the motions above mentioned are the immediate effect of electrical attraction and repulſion. The following amuſing experiments are performed by giving motion to bodies through the medium of air, i. e. by firſt putting the air in motion. Let the electrician provide himſelf with a ſet of vanes, made of gilt paper or tinſel, each about two inches in length and one in breadth. Let theſe be ſtuck in a cork, which may be ſuſpended from a magnet by means of a needle; and then, if they be held at a ſmall diſtance on one ſide of the end of a pointed wire proceeding from the conductor, they will be turned round with great rapidity by the current of the air which flows from the point. If the vanes be removed to the other ſide of the point, the motion will preſently ſtop, and begin again with the ſame rapidity, in a contrary direction; and thus the motion may be changed at pleaſure. This experiment may be diverſified by vanes cut in the form of thoſe of a ſmoke-jack; when, being held over the end of a pointed wire, turned upwards, and electrified, they will be turned round very ſwiftly, by the current of air flowing upwards. If they be held under a point projecting downwards, they will be turned the contrary way.

On the top of a finely pointed wire, riſing perpendicularly from the conductor, let another wire, ſharpened at each end, be made to move freely as on a center. If it be well balanced, and the points be bent horizontally, in oppoſite directions, it will, when electrified, turn very ſwiftly round, by the reaction of the air againſt the current which flows from the points. Theſe points may be nearly concealed, and horſes or other figures placed upon the wires, ſo as to turn round with them, and look as if the one purſued the other. This experiment Mr. Kinnerſley calls the ELECTRICAL HORSE RACE. If the number of wires proceeding from the ſame center be increaſed, and different figures be put upon them, the race will be more complicated and diverting. If this wire which ſupports the figures have another wire finely pointed riſing from its center, another ſet of wires, furniſhed with other figures, may be made to revolve above the former, and either in the ſame, or in a contrary direction, as the operator pleaſes.

If ſuch a wire, pointed at each end, and the ends bent in oppoſite directions, be furniſhed like a dipping needle with a ſmall axis fixed in its middle, at right angles with the bending of the points, and the ſame be placed between two inſulated wire ſtrings, near and parallel to each other, ſo that it may turn on its axis freely upon and between them; it will, when electrified, have a progreſſive as well as circular motion, from one end of the wires that ſupport it to the other, and this even up a conſiderable aſcent.

A VARIETY of beautiful appearances may be exhibited by means of electrical LIGHT, even in the open air, if the room be dark. Bruſhes of light from points electrified poſitively, and not made very ſharp, or from the edges of metallic plates, diverge in a very beautiful manner, and may be excited to a great length, by preſenting to them a finger, or the palm of the hand, to which they feel like ſoft lambent flames, which have not the leaſt pungency, nor give a diſagreeable ſenſation of any kind. It is alſo amuſing to obſerve the difference there is between bruſhes of light from pointed bodies electrified poſitively or negatively.

In the electrical horſe race above mentioned, a ſmall flame will be ſeen in the dark at every point of the bent wires; ſo that, if the operator can contrive to make the wire terminate in the horſe's tail, it will ſeem to be all on fire. And if a circular plate of metal be cut into the ſhape of a ſtar, ſo that every point may be at the ſame diſtance from the center, and the center be made to turn freely on a point, like the wires in the preceding experiment, a ſmall flame will be ſeen at every point; and if the ſtar be turned round, it will exhibit the appearance of a lucid circle, without any diſcontinuance of the light.

If the electric ſparks be taken from a braſs ball, at the extremity of a long braſs rod, inſerted into the prime conductor, they will often be ſeveral inches long, and iſſue in a great variety of crooked directions, exactly reſembling the courſe of lightning, and exhibiting a very amuſing ſpectacle. A friend of Dr. Franklin's ſuppoſes that the ſpark is thrown out of a ſtrait courſe by the denſity of the air increaſed by the action and reaction of the two fluids, which are repulſive of one another *.

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* Franklin's Letters, new edition, p. 167.

As the motion of the electric matter is, to the ſenſes, inſtantaneous, a variety of beautiful appearances may be exhibited by a number of ſmall electric ſparks, diſpoſed in various forms. This may be done by means of a board and a number of wires, in the following manner. Let two holes be made through the board, about a quarter of an inch on each ſide of the ſpot where a ſpark is deſired. Let the extremities of the wires neatly rounded, come through theſe holes, and be brought near together, exactly over the place; and let the wires on the back ſide of the board be ſo diſpoſed, as that an electric ſpark muſt take them all in the ſame circuit. When they are thus prepared, all the points will appear luminous at once, whenever a ſpark is taken by them at the prime conductor. In this manner may beautiful repreſentations be made of any of the conſtellations, as of the Great Bear, Orion, &c. and in this manner, alſo, may the outlines of any drawing, as of figures in tapeſtry, be exhibited.

The Abbé Nollet has taken a great deal of pains to make the appearance of letters, and other figures, by means of electric ſparks, and as it is impoſſible to make the ſparks follow one another in a complete circle, on the ſame ſide of any flat ſurface, he makes uſe of plates of glaſs, and places one half of the circle, &c. on one ſide of the plate, and the other half on the other ſide, connecting the pieces that are neareſt one another, but on different ſides of the glaſs, by wires brought round it*. The deſcription would be too tedious for this place, but the execution will be very eaſy, to any perſon who has but a little knowledge of electricity.

The force of an electric ſpark in ſetting fire to various ſubſtances was one of the firſt experiments that gave an *eclat* to electricity, and it is ſtill repeated with pleaſure. Spirit of wine a little warmed, is commonly made uſe of for this purpoſe. The experiment will not fail to ſucceed, if a pretty ſtrong ſpark be drawn, in any manner, or direction whatever, through any part of it; and this may eaſily be done many ways, if it be contained in a metal ſpoon with a pretty wide mouth. A candle newly blown out may be lighted again by the electric ſpark paſſing through the groſs part of the ſmoke, within half an inch of the ſnuff; though it is perhaps blown in again by the motion given to the air by the force of the exploſion. Alſo air produced by the effervеſcence of ſteel filings with oil of vitrol diluted with water, and many other ſubſtances, which throw out an inflammable vapour, may be kindled by it.

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* Lettres, Vol. iii. p. 281.

The ſtrong phoſphoreal or ſulphureous ſmell, which may be perceived by preſenting the noſtrils within an inch or two of any electrified point, makes a curious experiment, but it does not give a pleaſing ſenſation.

Lastly, the moſt entertaining experiment that can be performed by ſimple electrification, is when one or more of the company ſtand upon an inſulated ſtool, holding a chain from the prime conductor. In this caſe, the whole body is, in reality, a part of the prime conductor, and will exhibit all the ſame appearances, emitting ſparks wherever it is touched by any perſon ſtanding on the floor. If the prime conductor be very large, the ſparks may be too painful to be agreeable, but if the conductor be ſmall, the electrification moderate, and none of the company preſent touch the eyes, or the more tender parts of the face of the perſon electrified, the experiment is diverting enough to all parties.

Most of the experiments above mentioned may alſo be performed to the moſt advantage by the perſon ſtanding upon the ſtool, if he hold in his hand whatever was directed to be faſtened to the prime conductor. Spirit of wine may be fired by a ſpark from a perſon's finger as effectually as in any other way. Care muſt be taken that the floor on which the ſtool is placed be free from duſt, but it is moſt adviſable to have a large ſmooth board for the purpoſe.

SECTION II.

ENTERTAINING EXPERIMENTS PERFORMED BY MEANS OF THE LEYDEN PHIAL.

NO electrical experiments anſwer the joint purpoſe of pleaſure and ſurprize in any manner comparable to thoſe that are made by means of the Leyden phial. All the varieties of electrical attraction and repulſion may be exhibited, either by the wire, or the coating of it; and if the knobs of two wires, one communicating with the inſide, and the other with the outſide of the phial, be brought within four or five inches of one another, the electrical ſpider above mentioned will dart from the one to the other in a very ſurpriſing manner, till the phial be diſcharged. But the peculiar advantage of the Leyden experiment is, that, by this means, the electrical flaſh, report, and ſenſation, with all their effects, may be increaſed to almoſt any degree that is deſired.

When the phial, or the jar, is charged, the ſhock is given through a perſon's arms and breaſt, by directing him to hold a chain communicating with the outſide in one hand, and to touch the wire of the phial, or any conductor communicating with it, with the other hand. Or the ſhock may be made to paſs through any particular part of the body without much affecting the reſt, if that part, and no other, be brought into the circuit through which the fire muſt paſs from one ſide of the phial to the other.

A GREAT deal of diverſion is often occaſioned by giving a perſon a ſhock when he does not expect it; which may be done by concealing the wire that comes from the outſide of the phial under the carpet, and placing the wire which comes from the inſide in ſuch a manner in a perſon's way, that he can ſuſpect no harm from putting his hand upon it, at the ſame time that his feet are upon the other wire. This, and many other methods of giving a ſhock by ſurprize, may eaſily be executed by a little contrivance; but great care ſhould be taken that theſe ſhocks be not ſtrong, and that they be not given to all perſons promiſcuouſly.

WHEN a ſingle perſon receives the ſhock, the company is diverted at his ſole expence; but all contribute their ſhare to the entertainment, and all partake of it alike, when the whole company forms a circuit, by joining their hands; and when the operator directs the perſon who is at one extremity of the circuit to hold a chain which communicates with the coating, while the perſon who is at the other extremity of the circuit touches the wire. As all the perſons who form this circuit are ſtruck at the ſame time, and with the ſame degree of force, it is often very pleaſant to ſee them ſtart at the ſame moment, to hear them compare their ſenſations, and obſerve the very different accounts they give of it.

THIS experiment may be agreeably varied, if the operator, inſtead of making the company join hands, direct them to tread upon each others toes, or lay their hands upon each others heads; and if, in the latter caſe, the whole company ſhould be ſtruck to the ground, as it happened when Dr. Franklin once gave the ſhock to ſix very ſtout robuſt men, the inconvenience ariſing from it will be very inconſiderable. The company which the Doctor ſtruck in this manner neither heard nor felt the ſtroke, and immediately got up again, without knowing what had happened. This was done with two of his large jars (each containing about ſix gallons) not fully charged *.

The moſt pleaſing of all the ſurprizes that are given by the Leyden phial is that which Mr. Kinnerſley † contrived and called the MAGIC PICTURE, which he deſcribes in the following manner. Having a large metzotinto, with a frame and glaſs (ſuppoſe of the king) take out the print, and cut a pannel out of it, near two inches diſtant from the frame all round. If the cut be through the picture, it is not the worſe. With thin paſte, or gum water, fix the board that is cut off on the inſide of the glaſs, preſſing it ſmooth and cloſe: then fill up the vacancy, by gilding the glaſs well with leaf gold, or braſs. Gild likewiſe the inner edge of the back of the frame all round, except the top part, and form a communication between that gilding and the gilding behind the glaſs; then put in the board, and that ſide is finiſhed. Turn up the glaſs, and gild the foreſide exactly over the back gilding; and when it is dry, cover it, by paſting on the pannel of the picture that has been cut out, obſerving to bring the correſpondent parts of the board and picture together, by which the picture will appear of a piece as at firſt, only part is behind the glaſs, and part before. Laſtly, hold the picture horizontally by the top, and place a little moveable gilt crown, on the king's head

If now the picture be moderately electrified, and another perſon take hold of the frame with one hand, ſo that his fingers touch its inſide gilding, and with the other hand endeavour to take off the crown, he will receive a terrible blow, and fail in the attempt. The operator who holds the picture by the upper

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* Franklin's Letters, new edition, p. 324.

† Ibid. p. 29.

upper end, where the inſide of the frame is not gilt, to prevent its falling, feels nothing of the ſhock, and may touch the face of the picture without danger, which he pretends to be a teſt of his loyalty. If a ring of perſons take a ſhock among them, the experiment is called the CONSPIRATORS.

As the electric fire may be made to take whatever circuit the operator ſhall pleaſe to direct, it may be thrown into a great variety of beautiful forms. Thus, if a charged phial be placed at one extremity of the gilding of a book, and the diſcharge be made by a wire which touches the other extremity, the whole gilding will be rendered luminous. But if ſeveral pretty ſtrong ſhocks be ſent through the ſame gilding, they will ſoon render it incapable of tranſmitting any more, by breaking and ſeparating the parts too far aſunder. Alſo the electric conſtellations and figures, mentioned above, may be lighted up much more ſtrongly by a charged phial than by ſparks from the conductor; only, they cannot be lighted up ſo often in this way.

ON the ſame principle that the wires of phials charged differently will attract and repel differently, is made an ELECTRICAL WHEEL, which Dr. Franklin ſays, turns with conſiderable ſtrength, and of which he gives the following deſcription. A ſmall upright ſhaft of wood paſſes at right angles through a thin round board, of about twelve inches diameter, and turns on a ſharp point of iron, fixed in the lower end; while a ſtrong wire in the upper end, paſſing through a ſmall hole in a thin braſs plate, keeps the ſhaft truly vertical. About thirty *radii*, of equal length, made of ſaſh glaſs, cut in narrow ſlips, iſſue horizontally from the circumference of the board; the ends moſt diſtant from the center being about four inches apart. On the end of every one a braſs thimble is fixed.

IF now the wire of a bottle, electrified in the common way, be brought near the circumference of this wheel, it will attract the neareſt thimble, and ſo put the wheel in motion. That thimble, in paſſing by, receives a ſpark, and thereby being electrified, is repelled, and ſo driven forwards, while a ſecond being attracted approaches the wire, receives a ſpark, and is driven after the firſt; and ſo on, till the wheel has gone once round; when the thimbles before electrified approaching the wire, inſtead of being attracted, as they were at firſt, are repelled, and the motion preſently ceaſes.

BUT if another bottle, which had been charged through the coating, be placed near the ſame wheel, its wire will attract the thimble repelled by the firſt, and thereby double the force that carries the wheel round; and not only take out the fire that had been communicated by the thimbles to the firſt bottle, but even robbing them of their natural quantity, inſtead of being repelled when they come again towards the firſt bottle, they are more ſtrongly attracted; ſo that the wheel mends its pace, till it goes with great rapidity, twelve or fifteen rounds in a minute, and with ſuch ſtrength, that the weight of one hundred Spaniſh dollars, with which we once loaded it, did not in the leaſt ſeem to retard its motion. This is called an ELECTRICAL JACK, and if a large fowl was ſpitted on the upper ſhaft, it would be carried round before a fire, with a motion fit for roaſting.

BUT this wheel, continues the Doctor, like thoſe driven by wind, moves by a foreign force, to wit that of the bottles.

THE SELF MOVING WHEEL, though conſtructed on the ſame principles, appears more ſurpriſing. It is made of a thin round plate of window glaſs, ſeventeen inches diameter, well gilt on both ſides, all but two inches next the edge. Two ſmall hemiſpheres of wood are then fixed with cement to the middle of the upper and under ſides, centrally oppoſite; and in each of them a thick ſtrong wire, eight or ten inches long, which together makes the axis of the wheel. It turns horizontally, on a point at the lower end of its axis, which reſts on a bit of braſs, cemented within a glaſs ſalt cellar. The upper end of its axis paſſes through a hole in a thin braſs plate, cemented to a long and ſtrong piece of glaſs; which keeps it ſix or eight inches diſtant from any non-electric, and has a ſmall ball of wax or metal on its top, to keep in the fire.

In a circle on the table which ſupports the wheel, are fixed twelve ſmall pillars of glaſs, at about eleven inches diſtance, with a thimble on the top of each. On the edge of the wheel is a ſmall leaden bullet, communicating by a wire with the gilding of the upper ſurface of the wheel; and about ſix inches from it, is another bullet, communicating, in like manner, with the under ſurface. When the wheel is to be charged by the upper ſurface, a communication muſt be made from the under ſurface to the table.

When it is well charged, it begins to move. The bullet neareſt to a pillar moves towards the thimble on that pillar, and, paſſing by, electrifies it, and then puſhes itſelf from it. The ſucceeding bullet, which communicates with the other ſurface of the glaſs, more ſtrongly attracts that thimble, on account of its being electrified before by the other bullet, and thus the wheel increaſes its motion, till the reſiſtance of the air regulates it. It will go half an hour, and make, one minute with another, twenty turns in a minute, which is 600 turns in the whole, the bullet of the upper ſurface giving in each turn twelve ſparks to the thimbles, which makes 7200 ſparks, and the bullet of the under ſurface receiving as many from the thimble, thoſe bullets moving in the time near 2500 feet. The thimbles are well fixed, and in ſo exact a circle, that the bullets may paſs within a very ſmall diſtance of each of them.

If inſtead of two bullets, you put eight, four communicating with the upper ſurface, and four with the under ſurface, placed alternately (which eight, at about ſix inches diſtance, complete the circumference) the force and ſwiftneſs will be greatly increaſed, the wheel making fifty turns in a minute, but then it will not continue moving ſo long.

These wheels, the Doctor adds, may be applied perhaps to the ringing of chimes, and moving light made orreries *.

A phial makes the moſt beautiful appearances when it is charged without any coating on the outſide, by putting the hand, or any conductor, to it: for then, at whatever part of the jar the diſcharge is made, the fire will be ſeen to branch from it in moſt beautiful ramifications all over the jar, and the light will be ſo intenſe, that the minuteſt of the branches may be ſeen in open daylight.

The diſcharge of a large electrical battery is rather an awful than a pleaſing experiment, and the effects of it, in rending various bodies, in firing gun-powder, in melting wires, and in imitating all the effects of lightning, never fail to be viewed with aſtoniſhment. In order to fire gun-powder, it muſt be made up into a ſmall cartridge, with blunt wires inſerted at each end, and brought within half an inch of each other, through which the ſhock muſt paſs: or a very ſmall wire may be drawn through the center of it, and the exploſion will be made by its melting. A common jar will eaſily ſtrike a hole through a thick cover of a book, or many leaves of paper, and it is curious to obſerve the bur raiſed on both ſides, as if the fire had darted both ways from the center.

A considerable number of experiments with an electrical battery, ſome of which exhibit fine appearances, will be particularly deſcribed in the laſt part of the work.

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* Franklin's Letters, p. 28, &c.

SECTION III.

ENTERTAINING EXPERIMENTS MADE BY A COMBINATION OF PHILOSOPHICAL INSTRUMENTS.

IN order to exhibit ſome of the fineſt electrical experiments, the operator muſt call to his aid other philoſophical inſtruments, particularly the condenſing machine, and the air pump.

IF the fountain made by condenſed air be inſulated, and be made to emit one ſtream, that ſtream will be broken into a thouſand, and equally diſperſed over a great ſpace of ground, when the fountain is electrified; and by only laying a finger upon the conductor, and taking it off again, the operator may command either the ſingle ſtream, or the divided ſtream at pleaſure. In the dark, the electrified ſtream appears quite luminous.

THE greateſt quantity of electric light is ſeen *in vacuo*. Take a tall receiver very dry, and in the top of it inſert with cement a wire not very acutely pointed. Then exhauſt the receiver, and preſent the knob of the wire to the conductor, and every ſpark will paſs through the vacuum in a broad ſtream of light, viſible through the whole length of the receiver, be it ever ſo tall. This ſtream often divides itſelf into a variety of beautiful rivulets, which are continually changing their courſe, uniting and dividing again, in a moſt pleaſing manner. If a jar be diſcharged through this vacuum, it gives the appearance of a very denſe body of fire, darting directly through the center of the vacuum, without ever touching the ſides; whereas, when a ſingle ſpark paſſes through, it generally goes more or leſs to the ſide, and a finger put on the outſide of the glaſs will draw it wherever a perſon pleaſes. If the veſſel be graſped by both hands, every ſpark is felt like the pulſation of a great artery, and all the fire makes towards the hands. This pulſation is felt at ſome diſtance from the receiver; and in the dark, a light is ſeen betwixt the hands and the glaſs.

ALL this while the pointed wire is ſuppoſed to be electrified poſitively; if it be electrified negatively, the appearance is remarkably different. Inſtead of ſtreams of fire, nothing is ſeen but one uniform luminous appearance, like a white cloud, or the milky way in a clear ſtar-light night. It ſeldom reaches the whole length of the veſſel, but is generally only like a lucid ball at the end of the wire.

A VERY beautiful appearance of electric light in a darkened room may alſo be produced by inſerting a ſmall phial into the neck of a tall receiver, ſo that the external ſurface of the glaſs may be expoſed to the vacuum. The phial muſt be coated on the inſide, and while it is charging, at every ſpark taken from the conductor into the inſide, a flaſh of light is ſeen to dart, at the ſame time, from every part of the external ſurface of the jar, ſo as quite to fill the receiver. Upon making the diſcharge, the light is ſeen to return in a much cloſer body, the whole coming at once.

BUT the moſt beautiful of all the experiments that can be exhibited by the electric light is Mr. Canton's AURORA BOREALIS, of which the following is but an imperfect deſcription. Make a Torricellian vacuum in a glaſs tube, about three feet long, and ſeal it hermetically, whereby it will be always ready for uſe. Let one end of this tube be held in the hand, and the other applied to the conductor, and immediately the whole tube will be illuminated, from end to end; and when taken from the conductor, will continue luminous without interruption for a conſiderable time, very often above a quarter of an hour. If, after this, it be drawn through the hand either way, the light will be uncommonly intenſe, and without the leaſt interruption from one hand to the other, even to its whole length. After this operation, which diſcharges it in a great meaſure, it will ſtill flaſh at intervals, though it be held only at one extremity, and quite ſtill; but if it be graſped by the other hand, at the ſame time, in a different place, ſtrong flaſhes of light will hardly ever fail to dart from one end to the other; and this will continue twenty-four hours, and perhaps much longer, without freſh excitation. Small and long glaſs tubes exhauſted of air, and bent in many irregular crooks and angles, will, when properly electrified in the dark, beautifully repreſent flaſhes of lightning.

I shall conclude this deſcription of entertaining experiments with an account of the manner in which Dr. Franklin and his friends cloſed the year 1748. The hot weather coming on, when electrical experiments were not ſo agreeable, they put an end to them for that ſeaſon, as the Doctor ſays, ſomewhat humorouſly, in a party of pleaſure on the banks of the Skuylkil. Firſt, ſpirits were fired by a ſpark ſent from ſide to ſide through the river, without any other conductor than the water. A turkey was killed for their dinner by the electrical ſhock, and roaſted by the electrical jack, before a fire kindled by the electrified bottle, when the healths of all the famous electricians in England, Holland, France, and Germany, were drunk in electrified bumpers, under a diſcharge of guns from the electrical battery *.

Happy would the author of this treatiſe be to ſee all the great electricians of Europe, or even thoſe in England, upon ſuch an occaſion, and eſpecially after having made diſcoveries in electricity of equal importance with thoſe made in Philadelphia in the year referred to. With pleaſure would he obey a ſummons to ſuch a rendezvous, though it were to ſerve the illuſtrious company in the capacity of operator, or even in the more humble office of waiter. Chearfulneſs and ſocial intercourſe do, both of them, admirably ſuit, and promote the true ſpirit of philoſophy.

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* Franklin's Letters, p. 35.

PART VIII.

NEW EXPERIMENTS IN ELECTRICITY, MADE CHIEFLY IN THE YEAR 1766.

I SHALL, in the laſt part of this work, preſent my reader with an account of ſuch new experiments in electricity as this undertaking has led me to make. I hope the peruſal of this work may ſuggeſt many more, and more conſiderable ones to my readers, and then I ſhall not think that I have written in vain.

To make this account the more uſeful to ſuch perſons as may be willing to enter into philoſophical inveſtigations, I ſhall not fail to report the real views with which every experiment was made, falſe and imperfect as they often were. I was always greatly pleaſed with the extreme exactneſs and ſimplicity of Mr. Grey, and ſhall, therefore, imitate his artleſs manner. And though an account of experiments drawn up on this plan be leſs calculated to do an author honour as a philoſopher; it will, probably, contribute more to make other perſons philoſophers, which is a thing of much more conſequence to the public.

Many modeſt and ingenious perſons may be engaged to attempt philoſophical inveſtigations, when they ſee, that it requires no more ſagacity to find new truths, than they themſelves are maſters of; and when they ſee that many diſcoveries have been made by mere accident, which may prove as favourable to them as to others. Whereas it is a great diſcouragement to young and enterpriſing geniuſes, to ſee philoſophers propoſing that firſt, which they themſelves attained to laſt; firſt laying down the propoſitions which were the reſult of all their experiments, and then relating the facts, as if every thing had been done to verify a true preconceived theory.

THIS *ſynthetic* method is, certainly, the moſt expeditious way of making a perſon underſtand a branch of ſcience; but the *analytic* method, in which diſcoveries were actually made, is moſt favourable to the progreſs of knowledge.

I HAVE, indeed, endeavoured to make the whole preceding hiſtory of electricity uſeful in this view, by not contenting myſelf with informing the reader what diſcoveries have been made; but, wherever it could be done, acquainting him *how* they were made, and what the authors of them had in view when they made them. In general, this has not been difficult to do, the facts being recent, and moſt of the perſons concerned now living. And, perhaps, in no branch of ſcience has there been leſs owing to genius, and more to accident; ſo that no perſon, who will give a little attention to the ſubject, need be without hopes of adding ſomething to the common ſtock of electrical diſcoveries. Nay, it would be extraordinary, if, in a great number of experiments, in which things were put into a variety of new ſituations, no new fact, worth communicating to the public, ſhould ariſe.

THE method I propoſe will, likewiſe, give the moſt pleaſure to thoſe perſons, who delight in tracing the real progreſs of the human mind, in the inveſtigation of truth, and the acquiſition of knowledge; as I hope it will carry with it ſufficient evidence of its own authenticity. For this progreſs, we may aſſure ourſelves, has, in all caſes, been by eaſy ſteps, even when it has been the moſt rapid. Were it poſſible to trace the ſucceſſion of ideas in the mind of Sir Iſaac Newton, during the time that he made his greateſt diſcoveries, I make no doubt but our amazement at the extent of his genius would a little ſubſide. But if, when a man publiſhes diſcoveries, he, either through deſign, or through habit, omit the intermediate ſteps by which he himſelf arrived at them; it is no wonder that his ſpeculations confound others, and that the generality of mankind ſtand amazed at his reach of thought. If a man aſcend to the top of a building by the help of a common ladder, but cut away moſt of the ſteps after he has done with them, leaving only every ninth or tenth ſtep; the view of the ladder, in the condition in which he has been pleaſed to exhibit it, gives us a prodigious, but an unjuſt idea of the man who could have made uſe of it. But if he had intended that any body ſhould follow him, he ſhould have left the ladder as he conſtructed it, or perhaps as he found it, for it might have been a mere accident that threw it in his way. It is poſſible he had even better have deſtroyed it intirely; as, in ſome caſes, a perſon would more eaſily make a new ladder of his own, than repair an old and damaged one.

That Sir Iſaac Newton himſelf owed ſomething to a caſual turn of thought, the hiſtory of his aſtronomical diſcoveries informs us; and where we ſee him moſt in the character of an experimental philoſopher, as in his optical inquiries (though the method of his treatiſe on that ſubject is by no means purely analytical) we may eaſily conceive that many perſons, of equal patience and induſtry (which are not called qualities of the underſtanding) might have done what he did. And were it poſſible to ſee in what manner he was firſt led to thoſe ſpeculations, the very ſteps by which he purſued them, the time that he ſpent in making experiments, and all the unſucceſsful and inſignificant ones that he made in the courſe of them; as our pleaſure of one kind would be increaſed, our admiration would probably decreaſe. Indeed he himſelf uſed candidly to acknowledge, that if he had done more than other men, it was owing rather to a habit of *patient thinking*, than to any thing elſe.

I DO not ſay theſe things to detract from the merit of the great Sir Iſaac Newton; but I think that the intereſts of ſcience have ſuffered by the exceſſive admiration and wonder, with which ſeveral firſt rate philoſophers are conſidered; and that an opinion of the greater equality of mankind, in point of genius, and powers of underſtanding, would be of real ſervice in the preſent age. It would bring more labourers into the common field; and ſomething more, at leaſt, would certainly be done in conſequence of it. For though I by no means think that philoſophical diſcoveries are at a ſtand, I think the progreſs might be quickened, if ſtudious and modeſt perſons, inſtead of confining themſelves to the diſcoveries of others, could be brought to entertain the idea, that it was poſſible to make diſcoveries themſelves. And, perhaps, nothing would tend more effectually to introduce that idea, which is at preſent very remote from the minds of many, in which it ought to have a place, than a faithful hiſtory of the manner in which philoſophical diſcoveries have actually been made by others.

THAT this fidelity has been preſerved in the following narrative, I make no doubt of its being its own voucher. Its imperfections will be a ſufficient evidence. The ſame fidelity will alſo oblige me to relate ſeveral facts as appearing new to myſelf, which the courſe of the preceding hiſtory will ſhow to have been diſcovered by others, though I was not then aware of it. Of ſuch after-diſcoveries, however, I have mentioned only thoſe which, it will be ſeen, I have purſued ſomething farther than the original authors, having attended to circumſtances overlooked by them; or, at leaſt, having made the experiments with more exactneſs, ſo that the reader may expect ſomething really new under every article. And the experiments which prove the ſame thing will be found conſiderably different from thoſe of others, and to furniſh additional arguments of the ſame general propoſitions. This repetition of old diſcoveries, and this variety in the experiments by which they were made, were both occaſioned by a ſituation which is more or leſs common to every electrician in England; whereby we are ignorant of a great deal of what has been done by others.

IN the following narrative will alſo be found an account not only of experiments which are complete, which exhibit ſome new fact, and from which ſomething relating to the general theory of electricity may be deduced; but alſo ſome that are incomplete, which produced no new appearance, and from which nothing poſitive could be concluded. If electricians in general had done this, they would have ſaved one another a great deal of uſeleſs labour, and would have had more time for making experiments really new, and which might have terminated in conſiderable diſcoveries. Beſides, if things be really put into new ſituations, though nothing *poſitive* can be inferred from the experiment, at leaſt ſomething negative may; and this cannot be ſaid to be of no importance in ſcience; nor, ſtrictly ſpeaking, to be no new truth. A ſufficient number of theſe experiments may, in many caſes, lay a foundation for probable and poſitive concluſions.

I MAKE no apology for leaving ſo many of theſe experiments imperfect, and for publiſhing this account of them before they have been purſued ſo far as it may, perhaps, be thought they deſerve. I rather think the generality of philoſophers ought to make an apology to the public, for delaying the communication of their experiments and diſcoveries ſo long as they have done. It is poſſible I may never have any more leiſure or opportunity to purſue them, and others may better command both; whereby the diſcoveries will be ſooner brought to their maturity, and the progreſs of this branch of philoſophy accelerated. The genuine ſpirit of philoſophy is, ſurely, not that of mechanics, who make the moſt of every little improvement in their arts, and never divulge them, till they can make no more advantage of them themſelves. If I could this day communicate to any fellow-labourer a hint, which it was more probable he could immediately purſue to advantage than myſelf, I would not defer it till to-morrow. Nor do I think it is any great boaſt of a philoſophical indifference to fame to make this declaration. The great Sir Iſaac Newton ſeems to have had no idea of the purſuit of fame. He deferred the communication of his important diſcoveries through real modeſty, thinking it impertinent to trouble the public with any thing imperfect. I make no pretenſions to that kind of modeſty. Whether it be of a true or a falſe kind, I think it manifeſtly injurious to the progreſs of knowledge. Like thoſe who contended in one of the games of ancient Greece, I ſhall immediately deliver my torch to any perſon who can carry it with more dexterity. If others do the ſame, it may come into my hands again, ſeveral times, before we reach the goal.

IT may be ſaid, that I ought, at leaſt, to have waited till I had ſeen the connection of my new experiments with thoſe that were made before, and have ſhown that they were agreeable to ſome general theory of electricity. But when the facts are before the public, others are as capable of ſhowing that connection, and of deducing a general theory from them as myſelf. If but the moſt inconſiderable part of the temple of ſcience be well laid out, or a ſingle ſtone proper for, and belonging to it be collected; though at preſent it be ever ſo much detached from the reſt of the building, its connection and relative importance will appear in due time, when the intermediate parts ſhall be completed. Every *fact* has a real, though unſeen connection with every other fact: and when all the facts belonging to any branch of ſcience are collected, the ſyſtem will form itſelf. In the mean time, our gueſſing at the ſyſtem may be ſome guide to us in the diſcovery of the facts; but, at preſent, let us pay no attention to the ſyſtem in any other view; and let us mutually communicate every new fact we diſcover, without troubling ourſelves about the ſyſtem to which it may be reduced.

I THINK I ſhall give the moſt diſtinct view of the few things that I have obſerved, in the ſhort courſe of my electrical experiments, if I relate them pretty nearly in the order in which they occurred, only taking care not to intermix things of a very different nature. The earlieſt date in my experiments is the beginning of the year 1766; when, in conſequence of forming an acquaintance with ſome gentlemen who have diſtinguiſhed themſelves for their diſcoveries in electricity, and of undertaking to write the preceding hiſtory, my attention was firſt turned towards making ſome original experiments in this part of Natural Philoſophy, which had ſerved for my occaſional amuſement ſome time before.

SECTION I.

EXPERIMENTS ON EXCITATION, PARTICULARLY OF TUBES IN WHICH AIR IS CONDENSED, AND OF LARGE GLASS GLOBES.

FINDING by my own experiments, and thoſe of others, that a glaſs tube, out of which the air was exhauſted, diſcovered no ſign of electricity outwards, but that all its effects were obſerved on the inſide; I imagined that, if the air was condenſed in the tube, it would operate more ſtrongly on the outſide; ſo that an additional atmoſphere would give it a double virtue. But the reſult was the very reverſe of my expectations.

SOME time in the month of January, when the weather was dry and froſty, I took a glaſs tube, ſuch as is generally uſed for electrifying, about two feet and a half in length, and an inch in diameter. It was cloſed at one end, and by means of a braſs cap at the other, I fitted a condenſing engine to it; and when the tube was very dry, and in excellent order for making experiments, I began to throw in more air. At every ſtroke of the piſton I endeavoured to excite the tube, but found its virtue diminiſhed. It was obliged to be brought nearer than before to attract light bodies, and gave leſs light when rubbed in the dark; till, as near as I could judge, I had got one additional atmoſphere

ſphere into the tube, when its power was ſcarce diſcernible. Letting out the air by degrees, I obſerved it gradually recovered its power. It attracted light bodies at a greater diſtance; it gave louder ſnappings, and more light in the dark; and when the additional air was wholly let out, its power was immediately as great as it had been before any air was thrown in. This I tried ſeveral times with the ſame ſucceſs.

COMMUNICATING theſe experiments to Dr. Franklin and Dr. Watſon, they ſuggeſted to me, that the non-excitation of the tube above mentioned might be owing to moiſture introduced along with the air, and adhering to the inſide of the tube. This conjecture was rendered more probable by another experiment I had made in the mean time.

REPEATING my attempts to excite the tube above mentioned, I found that, after very hard rubbing, it began to act a little; and that its virtue encreaſed with the labour. Thinking it might be the warmth which produced this effect, I held the tube to the fire, and found that when it was pretty hot, it would act almoſt as well as when it contained no more than its uſual quantity of air. I conjectured that the warmth might expel the moiſture from the ſides of the glaſs, or make the encloſed air capable of holding a greater quantity of water in a ſtate of perfect ſolution.

WILLING to determine whether the additional quantity of air with the moiſture it occaſioned, acted in all reſpects like a non-electric coating, I tried the experiment with condenſed air, that Dr. Deſaguliers did with ſand. After condenſing the air, and finding the excitation of it impoſſible, as uſual, I let the air ſuddenly out, to ſee whether the tube would then ſhow any effect of the preceding friction; but it had not acquired the ſmalleſt degree of electricity; though the firſt ſtroke of the rubber, immediately afterwards, made it give ſparks to the finger at the diſtance of two and three inches. Perhaps the degree of moiſture it had contracted was very ſlight, and expelled by the act of excitation.

Upon being deſired to repeat this experiment with a particular view to the moiſture; I obſerved that not the leaſt cloudineſs could be perceived to adhere to the glaſs, at the time that it was abſolutely incapable of excitation. When one part of the tube was made warm, and the other left cold, the ſame ſtroke of the rubber would excite the warm part, without in the leaſt affecting the reſt. But ſtill the cold part of the tube appeared not in the leaſt more cloudy than that which was warm; and the moment the air was let out, the firſt ſtroke of the rubber made the whole ſtrongly electrical.

Willing to aſcertain whether the condenſing of air neceſſarily introduced more moiſture into a glaſs veſſel than the air could hold in perfect ſolution, I conſtructed a glaſs condenſer in ſuch a manner, that I could charge and diſcharge ſmall phials in the inſide of it; concluding, that if the additional air brought more additional moiſture, it would be impoſſible to charge a phial at all in thoſe circumſtances; whereas, if the air was free from moiſture, it would make the phial hold a greater charge, double in two atmoſpheres, treble in three, &c. Accordingly, I charged a tube about three quarters of an inch in diameter, and coated about eight inches, in the glaſs veſſel, containing about two atmoſpheres; and it received a much greater charge than it could be made to take in the open air, and as near as could be judged, by the report and flaſh, twice as great. At laſt the tube burſt by a ſpontaneous diſcharge, after being charged and diſcharged three or four times, in the condenſed air. It is not at all probable, that it could have been broke by any charge it could have held in the open air. This experiment ſeemed to determine, that there was no very great degree of moiſture introduced into the glaſs veſſel by the condenſation of air.

I AFTERWARDS found that experiments on condenſed air had been made by Mr. Du Fay and others, but not with all the circumſtances above mentioned.

SOME of my electrical friends are of opinion, that the reaſon why a tube with condenſed air in it cannot be excited is, that the denſe air within prevents the electric fluid from being forced out of the inſide of the tube, without which none can be forced into the outſide; and that heating the tube makes the air within leſs electrical, and the tube alſo; in conſequence of which, it more eaſily parts with the fluid on one ſide, and admits it on the other. But upon this principle how can a ſolid ſtick of glaſs be excited?

IMAGINING that a greater quantity of electric fire would be produced from the friction of larger globes than thoſe of the uſual ſize, I provided myſelf on the 24th of April 1766, with a globe ſeventeen inches and a half in diameter. It had only one neck, and was made exceedingly well; only being rather too large for the mouth of the furnace, a ſmall coal had ſtuck to its equatorial diameter, which, when it was ſtruck off, made a ſmall hole in it. This, in ſome meaſure, disfigured the globe, but I never imagined it could prevent its excitation in any great degree; ſo that I ſtill indulged hopes of acquiring, by its means, a prodigious power of electricity. But what was my ſurprize when, after I had got it mounted in the beſt manner poſſible, and after trying, for hours together, every method of friction, in the moſt favourable circumſtances for excitation, I could ſcarce get the appearance of fire from it; the ſparks from the the prime conductor being barely viſible.

ACQUAINTING Dr. Franklin with my diſappointment, he adviſed me to get the firſt coat of the globe taken off with emery;

as it had often been obſerved, that many globes would not work well, till after a conſiderable time, when the glaſs-houſe coat, as it may be called, is worn off. This operation I accordingly performed upon it, and incredibly laborious it proved; which greatly increaſed my diſappointment, when I found that it had all been labour in vain, for the globe had no more electrical power than before.

Dispairing of making any thing of this globe, I laid it aſide, and, on the 22d of May, got another, about fourteen inches in diameter. In blowing this globe, every circumſtance that I could imagine had, in the leaſt, contributed to my ill-ſucceſs with the former, was carefully avoided. The former was made late in the week, when the metal had been long in fuſion; becauſe I had been told, that globes made in that ſtate of the metal were always the beſt for electrical purpoſes. This was blown early in the week, when the workmen ſay the metal is moſt tranſparent, and freeſt from all kinds of imperfections. The former was warmed, in the courſe of making it, in a place in which wood and coals were frequently thrown, to keep up the heat. This was kept free from the fumes of any fuel whatever. Nothing could be finer than the metal of this globe, nothing more perfect in its form. It was alſo very well mounted, and I did not doubt of ſucceſs. But, after all, this globe, if poſſible, gave leſs fire than the former. I had recourſe to every method of excitation that I had ever heard of, or could myſelf imagine, but all in vain. The thing looked like enchantment.

Whilst I was thinking over every thing that I could imagine might poſſibly be the cauſe of my ill ſucceſs with theſe globes, I recollected, that another globe, which I had got made for a friend, in the ſame ſtate of the metal with my laſt, and only an inch and half leſs in diameter, acted exceedingly well; and that there was no other apparent difference between them, but that his had two necks, and an axis quite through it; whereas mine had only one neck, and no axis at all. Willing to try every thing, I reſolved to get the braſs cap of my globe perforated, and a ſmall wire introduced, to ſerve inſtead of an axis. This was done; but, in making the perforation, it happened, unfortunately, as I then thought, but the moſt fortunately in the world as it proved, that a lump of hard cement, about the bigneſs of a ſmall walnut was puſhed into the inſide of the globe. Vexatious as this circumſtance was, I was impatient to try my new experiment, and immediately began to whirl the globe, with this ſuccedaneum of an axis, though the cement was all the while rattling, and fouling the inſide.

I HAD not whirled the globe long, in theſe circumſtances, before I plainly perceived that its power increaſed. After ſome time it was pretty conſiderable, and I did not doubt but it was owing to the axis; nay I had formed a pretty plauſible theory, to account for an axis being neceſſary to a globe of ſuch a ſize. Willing, however, to verify the fact, and aſcertain my new hypotheſis, I took out the wire; but, to my ſurpriſe, found the virtue of the globe not at all diminiſhed. On the contrary, it continued increaſing, and by the time that the cement was well broken, and diſperſed, ſo as to have given a kind of lining to the globe, it power was exceedingly ſtrong, and it acted as well as any globe I had ever ſeen. In this ſtate, I obſerved, that after exciting any part of the ſurface, the ſmall pieces of cement in the inſide, to the diſtance of about two inches, would jump from the finger, or any conductor, preſented on the outſide.

HAVING, in this unexpected manner, made a perfect cure of this ſmaller globe; I remounted the larger, and conſidering, that the cement could probably act only as any other electric lining, I introduced into it ſome pounded ſulphur, mixed with ſome flower of brimſtone; and found that, as ſoon as there was enough to render it ſemi-opaque, it acted very well.

In this ſtate, the appearance of the globe was, in ſeveral reſpects, very remarkable. The part that was rubbed had none of the ſulphur upon it, except thoſe places where the poliſh had been, in ſome meaſure, taken off by the emery, in the firſt operation. Theſe being circular, the brimſtone lay upon them, like the belts of Jupiter. The hemiſphere oppoſite to the neck had twice as much ſulphur upon it as the other; and, in both hemiſpheres, the ſulphur lay thicker, as it receded from the equatorial diameter.

I afterwards put as much more ſulphur into it, which doubled the lining equally every where; but left two or three great heaps, in particular parts of the equatorial diameter, where it was rubbed, and where I could perceive no defect of poliſh. Whirling the globe, upon this, I found the virtue almoſt quite gone, and even the amalgam could not revive it. Endeavouring to take the ſulphur out of the globe, I broke a great hole into it; and alſo the new globe was broken the ſame day, by a lump of hard cement, in the inſide, falling from the top to the bottom. Theſe accidents rendered my experiments incomplete.

I then propoſed to get another large globe, with one neck, and a large hole in the oppoſite ſide; by means of which I could eaſily put different ſubſtances into it, and take them out again, in order to find the cauſe of the appearances above mentioned. But apprehending this courſe of experiments might prove a little too expenſive, and, after all, terminate in nothing, I unwillingly deſiſted.

I shall add to this ſection, on the ſubject of excitation, that I once whirled a very thin globe, about ſix or ſeven inches in diameter, which was made to weigh air, and not one fourth part ſo thick as a common Florence flaſk. It was excited very powerfully by a piece of leather which had been ſoaked in a mixture of tallow and bees wax, and into which a quantity of amalgam had been worked. With this globe I could make my common jar diſcharge itſelf over more than five inches of the external ſurface, which I reckon to be a conſiderable proof of its power. It ſeems to follow from this experiment, that the thinneſs of glaſs globes, or tubes, is by no means any obſtruction to their electric power.

IN the courſe of theſe experiments I had read Mr. Bergman's account of his curing a globe by a lining of melted ſulphur, and had propoſed to try that in the laſt place, on account of the diſagreeable operation; but found it ſuperſeded in the manner deſcribed above.

SECTION II.

EXPERIMENTS WHICH PROVE A CURRENT OF AIR FROM THE POINTS OF BODIES ELECTRIFIED EITHER POSITIVELY OR NEGATIVELY.

DURING a courſe of electrical experiments, made to divert ſome of my friends, one of the company happened to preſent a pointed wire to my hand, as I was ſtanding upon an inſulated ſtool; when I was ſurpriſed to perceive a cool blaſt proceeding from it; though, according to Dr. Franklin's theory, the current of the fluid went from my hand to the point. I then preſented my noſtrils to the point, and perceived the ſame ſtrong phoſphoreal ſmell, as if the point had been electrified poſitively. Theſe facts made me entertain ſome doubts about the direction of the current, and the principles of Dr. Franklin's theory, and led me to the following courſe of experiments; which prove nothing againſt that theory, but eſtabliſh a real current of air from the points of all electrified bodies.

CONSIDERING that flame is the leaſt ſenſibly affected with electrical attraction or repulſion, but moſt eaſily with the leaſt breath of air; and not doubting at that time, but that the current of air would be in the direction of the fluid, being, as it were, impelled by it; I preſented the flame of a candle to a pointed wire, electrified negatively, as well as poſitively. The blaſt was ſo ſtrong (in both caſes alike) as to lay bare the greateſt part of the wick, the flame being driven from the point; and ſometimes a pretty large candle would be actually blown out by the blaſt. But, in all caſes, the effect was the ſame whether the electric fluid iſſued out of the point, or entered it.

Placing the flame between two points, one of which communicated with the prime conductor electrified poſitively, and the other with the floor, the flame was blown from that which communicated with the conductor upon the other, but not to ſo great a diſtance as if the other had been away. Changing the points, the effect was ſtill the ſame, whether that which communicated with the conductor was the more ſharp, or the more blunt of the two, the flame always receding from it.

Reversing this experiment, and making one of the points communicate with the rubber, and the other with the floor, the flame was always blown from the former towards the latter. It was evident, however, that the point which communicated with the floor had a current of air blowing from it likewiſe; for it counteracted the other, and would, when brought near the flame, raiſe it almoſt perpendicular, when it had been blown quite aſide by the other.

Placing the flame between two points, one of which communicated with the rubber, and the other with the conductor, it was equally affected by both, being always blown from the point which was neareſt to it.

It was very obſervable, that, notwithſtanding the current of air from the points affected the flame ſo remarkably; yet a ſmall portion of it, when it was brought very near the point, would be ſtrongly attracted by it, at the ſame time that the greateſt part of the flame was, by the current of air, blown the contrary way. This effect was always the ſame, whether the point was electrified poſitively or negatively; though, I fancied that the negative point attracted the flame more ſenſibly than the other.

Afterwards I diverſified this experiment in the following manner. I charged the inſide of a ſmall jar poſitively, then ſetting it upon a glaſs ſtand, in contact with a pointed wire, I placed the flame of a candle within an inch of the point, and touched the wire of the jar, with a braſs rod which I held in my hand. At every touch the flame was blown ſtrongly from the point. Sometimes it would be blown out; but another point being held oppoſite to it, would ſupport the flame; and more ſtrongly, if that point was joined with the rod with which I touched the wire of the jar. Charging the jar negatively in the inſide, all the effects were the very ſame. Diſcharging the jar through the points, with the flame in a right line between them, it was diſturbed, but not blown to one ſide more than the other.

To take off all the effect of electrical attraction and repulſion, and leave the current of air to act ſingly, I interpoſed pieces of braſs wire communicating with the earth, between the points of the wire and the flame; and found the blaſt to be rather increaſed than diminiſhed thereby.

Having communicated theſe experiments to Dr. Franklin, he adviſed me to try the force of this current upon *paper vanes*, ſuch as he has deſcribed in his letters: for, with him they ſeemed to turn one way or the other indifferently, juſt as they happened to ſet out. Accordingly I took a cork, and ſtuck into the ſides of it thirteen vanes, each being half a card, well dried, and each proceeding from the center of the cork. Into the cork I ſtuck a needle, by which I ſuſpended the whole on a magnet.

These vanes I held two or three inches from the point of a wire, communicating with the outſide coating of the jar, placed upon an electric ſtand, in the manner deſcribed above; and obſerved, that whenever I took a ſpark from the wire communicating with the inſide, the vanes were ſtrongly blown upon, and made to turn, as if the current of air had flowed from the point; at the ſame time that, according to Dr. Franklin's theory, the electric fluid was entering it. If they were made to turn the contrary way, the current ſoon ſtopped them, and never failed to bring them back, and make them move as before.

WHEN wires communicating with the floor were placed between the vanes and the point, to take off all the electrical attraction and repulſion, the vanes ſtill moved as briſkly as ever.

WHEN the jar was charged pretty high, the motion might be made ſo ſwift, that the ſeparate vanes could hardly be diſtinguiſhed, as the whole ſet turned round.

I MOREOVER obſerved, that the vanes were turned very briſkly, not only when held near the point, but alſo when held any where within the diſtance of ſix or ſeven inches from the ſides of the wire, which I made ſometimes of a conſiderable length. The ſtream would turn the vanes one way on one ſide of the wire, and the contrary way on the other; and being removed quickly to the different ſides, the direction of their courſe might be changed ſeveral times, in the diſcharge of one ſmall jar.

I MADE points to project two ways at the ſame time, and obſerved, that the ſtream was the ſame from both, and alſo when the points were made to project at right angles from one another. In this poſition of the wires, it was amuſing to obſerve, that the vanes would move one way, when held near one of the wires; and immediately turn about and move the contrary way, if removed near the other.

HITHERTO I had made my vanes of very dry paper, in order to make them leſs affected by electrical attraction and repulſion, that ſo the current of air might be the more indiſputable; but Mr. Canton deſiring me to try vanes that were conductors, I firſt dipped my paper vanes in water, and afterwards made a ſet of tinſel, or thin pieces of braſs, of the ſame form with the other. Theſe vanes, being conductors of electricity, promoted a freer current of the electric matter, and conſequently, occaſioning a greater motion to be given to the air, they whirled about with more rapidity than the former. When they were inſulated, they were affected juſt as the dry paper vanes had been.

With theſe vanes, I diverſified the experiment in a manner which ſhowed the ſameneſs of the current, notwithſtanding the change of electricity, in a clearer manner than before. I inſulated a jar, with a wire projecting from the coating, and held the tinſel vanes near the extremity of it. All the time the jar was charging, the vanes turned with great rapidity, as if by a blaſt from the point. Keeping the jar, the pointed wire, and the vanes in the ſame ſituation, the gradual diſcharge of the jar, made by now and then touching the wire which communicated with the inſide, made the vanes ſtill turn the ſame way, and, as far as could be perceived, with the ſame force.

To diverſify this experiment, I placed a charged jar upon a ſtool which had glaſs feet, a pointed wire projecting from the coating, a quantity of braſs duſt before the point, and a braſs chain communicating with the ground on the other ſide of the duſt. In this ſituation every attempt to diſcharge the jar threw a conſiderable quantity of the duſt from the point, being raiſed about ſeven or eight inches, and blown to a conſiderable diſtance. Removing the pointed wire from the coating of the jar, and connecting it with the chain, the ſame attempt to diſcharge it blew the duſt upon the jar. Uſing two points, one at the jar, and the other at the chain, the duſt was diſturbed, and raiſed up, but not blown one way more than the other. Fine flour anſwered nearly as well.

LASTLY, I made the experiment of the current with vanes in the form of a ſmoke jack, which anſwered as well as the others. They were moved when held more than a foot above the point, and likewiſe at a conſiderable diſtance below it, when it was turned downwards.

AFTER theſe experiments, I read in Mr. Wilſon's treatiſe on electricity, that the vanes would not turn in vacuo. This I tried, and found it to be true, and at the ſame time I found, they would not turn in a cloſe receiver, not exhauſted, where the air was confined, and had not a free circulation.

THE current of air from the points of bodies electrified *plus* or *minus*, is not more difficult to be account for on Dr. Franklin's hypotheſis of poſitive and negative electricity, than any other caſe of electrical repulſion. The particles of the atmoſphere, near the points of electrified bodies, having, by their means, become poſſeſſed of more or leſs than their natural ſhare of the electric fluid, muſt, according to the rule above mentioned, retire to places where they can diſcharge or repleniſh themſelves, as occaſion may require. If it be aſked why the particles of the atmoſphere do not, in the ſame manner, recede from all the parts of the electrified body, as well as from the points; it is anſwered, that, as the preſſure of the atmoſphere will prevent a vacuum, and as electrical attraction and repulſion are moſt powerful at the points of bodies, on account of the eaſier entrance or exit of the fluid at the points (upon whatever principle that effect depends) the electrified atmoſphere (whether negative or poſitive makes no difference) muſt fly off at the points preferably to any other places, and the weight of the atmoſphere will force the air of the neighbouring places upon the flatter parts of the electrified conductor, notwithſtanding the real endeavour it may have to recede from it.

SECTION III.

EXPERIMENTS ON FIXED AIR, AND CHARCOAL.

DR. FRANKLIN, to whom I had communicated ſome imperfect experiments on the electricity of noxious air, recollected them when he was this laſt ſummer at Pyrmont, where a large body of fixed air always lies upon the ſurface of the medicinal ſpring (for this air is evidently ſpecifically heavier than common air, and does not eaſily mix with it) but not having a proper apparatus, and the company there making experiments inconvenient, he did nothing that was deciſive; though, from the little that he had an opportunity of doing, he imagined it was not a conductor: and I have ſince found that this ſuppoſition was juſt. A charged phial may be dipped into a body of fixed air, reſting on the ſurface of a fermenting vat, without being diſcharged. If two equal phials, however, be equally charged at the ſame time, and one of them be plunged into the fixed air, and the other kept out of it; the latter will always retain the charge longer than the former, which will ſometimes retain it but a very ſhort time; owing, as I ſuppoſe, to the *moiſture*, which is readily abſorbed by the fixed air.

I ALSO found *inflammable* air to be the ſame as common, or fixed air, with reſpect to the power of conducting electricity.

THESE experiments on fixed air, imperfect as they were, led, however, to a diſcovery, which may poſſibly throw ſome new light upon ſome of the moſt fundamental principles of electricity.

BEING at that time but little acquainted with the nature of air, imagining, that fixed air only was unfit for reſpiration, and knowing that air was moſt injured by burning *charcoal*, I thought of trying charcoal itſelf in ſubſtance. Accordingly, on May the 4th, 1766, I tried charcoal, in a variety of ways and ſtates; and found it to be, what I had ſuſpected, an excellent conductor of electricity.

PRESENTING a piece of charcoal to the prime conductor, together with my finger, or a piece of braſs wire, I conſtantly obſerved, that the electric ſpark ſtruck the charcoal before either of the other conductors, if it happened to be advanced ever ſo little before them. Having a very rough ſurface, the charcoal did not take a denſe ſpark from the conductor, till it was made a little ſmooth, and brought within about half an inch; when, to all appearance, it did quite as well as any piece of metal, there being a conſtant ſtream of denſe and white electric fire between the conductor and it. I tried the charcoal in every ſtate of heat or cold, and found no alteration of its conducting power.

I PLACED a great number of pieces of charcoal, not leſs than twelve or twenty, of various ſizes, in a circuit, and diſcharged a common jar through them; when, to all appearance, the diſcharge was as perfect, as if ſo many pieces of metal had been placed in the ſame manner. Two of the pieces, about the middle of the circuit, I placed about an inch and a half from one another; but, upon the diſcharge, the ſpark paſſed the interval very full and ſtrong. A piece of charcoal alſo made the diſcharge at the wire with one ſpark, but the report was not ſo loud as when the diſcharge was made with a piece of metal. It was obſervable, that a black groſs ſmoke roſe from between each of the pieces of charcoal, at the moment of the diſcharge; but the ignition was momentary, and the fire could not be perceived on the charcoal.

To make the experiment of the conducting power of charcoal in the moſt indiſputable manner, I took a piece of baked wood, which I had often uſed for the purpoſe of inſulation, being an excellent non-conductor, and putting it into a long glaſs tube, I thruſt it into the fire, and converted it into charcoal. In this operation, a very great quantity of groſs ſmoke roſe from it, ſo that, ſeeming to part with more of its moiſture, one would have expected it would have come out a better non-conductor; but, upon trial, its electric property was quite gone, and it was become a very good conductor.

The experiments above mentioned were firſt made with *wood charcoal*, of which I found pieces of very different degrees of conducting power; but the moſt perfect conductors I have found of this kind are ſome pieces of *pit charcoal*. Theſe ſeem to be, in all reſpects, as perfect conductors as metals. They receive a ſtrong bright ſpark from the prime conductor, though ſeldom at above an inch diſtance, on account of the roughneſs on their ſurface, which cannot be taken off; and in diſcharging a jar through them, or with them, no perſon can imagine any difference between them and metal, either in the colour of the electric ſpark, or the ſound made by the exploſion. When they are broken, they exhibit an appearance which very much reſembles that of broken ſteel. There is however a great variety in the electrical properties of different pieces of this kind of charcoal; and for want of proper opportunity I have not yet ſucceeded in aſcertaining, with ſufficient certainty,

tainty, the circumſtances, in the preparation, &c. on which this variety depends.

I WOULD have preferred the examination of wood charcoal on many accounts; particularly, as the ſame ſubſtance is, in this caſe, converted from a perfect electric to a perfect conductor; and all the degrees of conducting power may be found in different ſpecimens of it; whereas pit coal is itſelf a conductor, though an imperfect one: but not having any opportunity, I procured ſpecimens of all the varieties I could imagine in the ſame heap of pit charcoal, with reſpect to their nearneſs or diſtance from the ſurface, &c. but though I examined them with all the care and attention that I could apply, and in every method that I could think of, the differences were ſo exceedingly ſmall, if any, that I could not fix upon any circumſtance that I could depend upon for the cauſe of them.

EVEN common cinders from an open fire, of the kind of coals which we generally burn, I find to be very little inferior to charcoal; which is ſuffered to flame, but covered very cloſe as ſoon as it is well burnt, and before any aſhes are formed. Coals and cinders from a common fire, being a very commodious ſubject for experiments, I did not fail to make as many upon them as I could imagine would be of any uſe; except that I had no opportunity of trying a ſufficient variety of coals. I took ſeveral out of the fire after they had done blazing, ſome of which I covered with aſhes, ſome I quenched in water, and ſome I left to cool in the open air. I alſo reduced ſome of the coals to cinders in a glaſs veſſel, without ſuffering them to flame; and I treated in the very ſame manner various pieces of oak, cut from the ſame plank; but when I examined them, I found their differences, with reſpect to their power of conducting electricity, very inconſiderable, if any. I thought the cinder of a coal which we call kennel, and which is remarkable for flaming much while it burns, to be a better conductor than a cinder from a common coal: but the difference might be owing to its more uniform texture, and ſmoother ſurface. Charcoal made of coals which yield a ſtrong ſulphureous ſmell when they are burnt, and of which the charcoal itſelf is not quite diveſted, was, to all appearance, as good a conductor as that of the other kind, which is more eſteemed.

In this courſe of experiments I found myſelf much at a loſs for a ſufficiently accurate method of aſcertaining the difference of conducting ſubſtances, and I wiſh that electricians would endeavour to find ſuch a meaſure. One of the beſt that I am acquainted with, and which I applied among others on this occaſion, is by the reſiduum of diſcharges, meaſured by Mr. Lane's electrometer. It is well known, that the worſe the conductors are that form the circuit, the greater the reſiduum will be left in a jar after a diſcharge; and Mr. Lane's electrometer, which meaſures an exploſion, will likewiſe meaſure the reſiduum. To apply this method with accuracy, I put pieces of charcoal, &c. of the ſame length into the circuit, I uſed the very ſame chain in every experiment, and the ſame diſpoſition of every part of the apparatus; I alſo made the exploſions exactly equal, and after every diſcharge completed the circuit by the chain before I took the reſiduum; and laſtly, I was careful to take up the ſame time in each operation, which I repeated very often. This method of meaſuring the conducting power of ſubſtances I learned of Mr. Lane.

In the proſecution of theſe experiments on charcoal, I burned a piece which I had found to be a moſt excellent conductor, firſt between two crucibles, and then in the open fire, and tried it at different times till it was almoſt burned away; but, contrary to my expectations, I found its property very little diminiſhed. I was, likewiſe, ſurpriſed to find that *ſoot*, whether of wood coal, or pit coal, hardly conducted at all. I made five or ſix inches of the ſoot of pit coal part of the electric circuit, which completed the communication between the inſide and outſide of a charged jar for ſeveral ſeconds; and yet found the charge not much diminiſhed. A piece of wood ſoot, which is a firm ſhining ſubſtance, which does not ſoil the fingers, and which ſeems to break in a poliſh in ſeveral places, would hardly conduct any part of a charge in the leaſt ſenſible degree. When rubbed againſt my hand, or my waiſtcoat in froſty weather (though it was difficult to find any part of it that was large and ſmooth enough for the purpoſe) I more than once thought it attracted the thread of trial. The ſnuff of a candle would not conduct a ſhock, though it was placed in the middle of the circuit, and it was eaſily ſet on fire by the exploſion of a ſmall jar.

But notwithſtanding my want of ſucceſs, I make no doubt, but that any perſon of tolerable ſagacity, who has an opportunity of making experiments in a laboratory, where he could reduce to a coal all kinds ſubſtances, in every variety of method, might very ſoon aſcertain what it is that makes charcoal a conductor of electricity. In all the methods in which I could make charcoal, the fume of the bodies was ſuffered to eſcape; but let trials be made of ſubſtances, reduced to a coal without any communication with the open air, or where the vapours emitted from them ſhall meet with different degrees of reſiſtance to their eſcape, aſcertained by actual preſſure.

Charcoal, beſides its property of conducting electricity, is, on many other accounts, a very remarkable ſubſtance; being indeſtructible by any method, beſides burning in the open air; and yet it ſeems not to have been ſufficiently ſtudied by any chymiſt. A proper examination of it promiſes very fair, not only to aſcertain the cauſe of its conducting, and, perhaps, of all conducting powers; but to be an opening to various other important diſcoveries in chymiſtry and Natural Philoſophy; and the ſubject ſeems to be fairly within our reach.

Pit coal, and probably all other ſubſtances, at the ſame time that they loſe much of their weight, increaſe conſiderably in their bulk in the operation of charring. Does it not ſeem to follow from hence, that its conducting power may poſſible be owing to the largeneſs of its pores, agreeable to the hypotheſis of Dr. Franklin, that electric ſubſtances have exceeding ſmall pores, which diſpoſe them to break with a poliſh.

Or, ſince the calces of metals, which are electric bodies, become metals, and conductors, by being fuſed in contact with charcoal; are not metals themſelves conductors of electricity, in conſequence of ſomething they get from the charcoal?

This courſe of experiments, however, evidently overturns one of the earlieſt, and, hitherto, univerſally received maxims in electricity, viz. that *water* and *metals* are conductors, and all other bodies non-conductors: for we have here a ſubſtance, which is clearly neither water, nor a metal, and yet a good conductor.

N. B. I have ſince found that it is the *degree of heat* with which charcoal is made, on which the *degree of its conducting power* depends. An account of this and other obſervations on charcoal may be ſeen in a paper of mine on the ſubject *.

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* Phil. Tranſ. Vol. lx. p. 211.

SECTION IV.

Experiments on the CONDUCTING POWER of various substances.

FINDING ſome contrariety of opinion among electricians about the nature of ICE, ſome ſaying it was a conductor of electricity, and others a non-conductor, ſo as even to be capable of being charged like glaſs, I took the opportunity of a pretty ſevere froſt, in the month of February, to aſſure myſelf of the fact.

In order to this, I took a large piece of ice, waſhed it very clean, and ſcraped off all the ſharp points about it. After this, when it was again perfectly frozen, I inſulated it, at night, in the open air, whither I had carried my machine on purpoſe, at the ſame time that it was freezing intenſely.

When, by drawing a feather over its ſurface, I found it be perfectly dry, I electrified it, and fetched large ſparks, not leſs than an inch in length, from all parts of it. I charged a jar at it, almoſt as well as at the prime conductor; I alſo diſcharged the jar through it, and along the ſurface of it, in ſeveral places; ſo that I had no doubt, but that ice was, nearly, as good a conductor of electricity as water. To try the ſame to more advantage, I took a charged jar into the open fields; and, by means of a great length of chain, diſcharged it along a large ſurface of ice on a pond, whilſt the ſurface was very dry, and the froſt continued very intenſe. But the ice being not ſo good a conductor as metal, if the chain communicating with the outſide of the jar happened to lie five or ſix inches from the knob of the wire communicating with the inſide, the fire would ſtrike to the chain, along the ſurface of the ice, without entering it.

SNOW is evidently not ſo good a conductor as ice; probably becauſe its parts do not lie in contact with one another, as thoſe of ice.

FINDING alſo that electricians were not perfectly agreed about the conducting power of *hot glaſs*, and that the methods which had been uſed to prove it were liable to objection; ſince, when the electricity was communicated along the outſide of the glaſs, it might be ſaid that the *hot air*, and not the hot glaſs was the conductor; it occurred to me, that the following experiment would determine this affair, in a more ſatisfactory manner than it had hitherto been done.

I PROCURED a glaſs tube, about four feet long; and, by means of mercury in the inſide, and tinfoil on the outſide, I charged about nine inches of the lower part of it. Then carefully ſlipping off the tinfoil, and pouring out the mercury, I heated the charged part of the glaſs red-hot; and found, upon replacing the coating, that it was diſcharged.

I MADE the experiment a ſecond time, with the ſame ſucceſs; ſo that I had no doubt, but that glaſs, when red-hot, was pervious to the electric fluid. It could not have gone round from the inſide to the outſide, without going over a ſurface of ſix feet of glaſs, the greateſt part of which was kept very cold, and all of it exceedingly dry.

THAT the charge had not been loſt by changing the quickſilver was evident: for when I repeated that part of the experiment, without heating the glaſs, the charge was found to be very little diminiſhed.

SOME time after, when I was preparing ſome baked wood for the purpoſes of inſulation, I found, that if I uſed them ſoon after they were taken out of the oven, they would not anſwer my purpoſe at all. The electricity went off by them to the floor. But when they had ſtood, in the very ſame ſituation, till they were cold, they inſulated very well.

UPON this, I made a piece of baked wood, which I had formerly uſed for inſulation, pretty hot; and when it was ſo hot, that I could hardly hold it in my hand, it took a ſlender ſpark from the conductor, about an inch long; but it would not diſcharge a jar at once. It did it however ſilently, pretty much like moiſt wood.

THE conſideration of the conducting power of charcoal, and the manner in which it is made, namely, by burning inflammable ſubſtances, in a cloſe place, and generally without flaming, led me to make a few experiments on the conducting power of the effluvia of flaming bodies, at the very time of their emiſſion: for whatever thoſe effluvia be, they ſeemed in ſome meaſure to contain the conducting principle.

THE conducting power of the flame of a candle was obſerved very early; but it was not compared with that of other things, and it had by ſome been ſuppoſed to be nothing more than the heat communicated to the neighbouring air. The experiments I am going to recite ſeem to overturn this hypotheſis.

MARCH the 14th, a ſmall charged phial held not longer than a ſecond within two or three inches of the flame of a candle, either above or below it, where the heat was altogether inconſiderable,

ſiderable, and the rarefaction of the air in a manner nothing, was totally diſcharged. The event was the ſame when I uſed the flame of a wax candle, or the flame of ſpirit of wine. When it was held much nearer to a red-hot poker, it was not diſcharged near ſo ſoon; and when it was held exceedingly near to a piece of red-hot glaſs, it was not diſcharged at all, except by one exploſion, ſeemingly conducted by the hot glaſs. Similar experiments were made by placing the candle, the poker, and the hot glaſs near the prime conductor. It was alſo found, that the ſmall phial above mentioned could not be diſcharged in the focus of a concave mirror.

But the ſmall jar above mentioned was diſcharged in theſe experiments ſilently; and though they ſeemed to be clearly in favour of the conducting power of the effluvia, which paſs off in flame, there was nothing very ſtriking in them; but afterwards, when I had conſtructed an electrical battery, I repeated the experiments in a much more ſtriking and convincing manner.

December the 15th, I brought the flame of a candle between two braſs knobs, one communicating with the inſide, and the other with the outſide of the battery; and obſerved, that as the flame advanced towards them, it began to be put into a quivering motion, exceedingly quick, and was ſtrongly drawn both ways towards each knob, leaving the wick bare at the top; and as ſoon as the flame was quite between the rods, the battery diſcharged at once, at the diſtance of three inches and an half. This is a very fine experiment. The interpoſition of the flame between the two braſs rods is like putting fire to a train of gunpowder, which explodes immediately.

When I advanced the ignited wick of a candle, juſt blown out, towards the rods, it was ventilated very briſkly; and when it was put between them, when ſeparated to about the diſtance of an inch, the diſcharge was made, and the candle blown in again.

To compare the conducting power of flame with that of other bodies, which had more heat but leſs effluvia, I put a red-hot poker between the two rods, but it did not promote the diſcharge of the battery till they were brought within about an inch and an half of one another; ſo that the exploſion was made at about twice the uſual diſtance, allowing for the ſpace occupied by the poker itſelf; and yet the air in the neighbourhood of the poker was more than ten times hotter than in the neighbourhood of the candle, conſidering the diſtance at which they were held from the rods. Both ſides of the hot poker were marked with an imperfect circle, like thoſe that were impreſſed on each of the knobs; an account of which will be given hereafter.

I THEN interpoſed a piece of red-hot glaſs, which has as great a heat as the iron, but emits leſs effluvia; but it did not promote the diſcharge till the braſs rods were brought within an inch of one another, which was ſo near, that the glaſs almoſt touched them both.

As I was diverſifying the experiments concerning the paſſage of the electric exploſion over the ſurfaces of various bodies, as will be mentioned hereafter, I accidentally diſcovered how exceedingly poor a conductor is *oil* of every kind; inſomuch that I think it ought rather to be claſſed among electric ſubſtances; though before that time, I imagined that oil did not differ very much from water, with reſpect to it conducting power. I had been led into the miſtake by ſome experiments of Mr. Wilſon, who has ſomewhere advanced the propoſition above mentioned; and argues that the tourmalin is poſſeſſed of a fixed kind of electricity, incapable of being conducted away, becauſe it retains the ſeparate power of each of its ſides, though ſurrounded with melted greaſe; whereas I find, that nothing of an oily nature will conduct electricity.

LAYING a chain, which communicated with the outſide of my battery, in a diſh of melted tallow, I brought a braſs rod communicating with the inſide towards it, in order to make the diſcharge, by tranſmitting the exploſion over the ſurface without entering it; when I was ſurpriſed to find, not only that the electric matter would not take the ſurface, but that, though it attracted a column of tallow at the diſtance of about three quarters of an inch (which was thicker in proportion as the rod was brought near the ſurface) and though I continued amuſing myſelf with this column of tallow a conſiderable time; in which ſtate it formed a complete communication between both ſides of the battery, yet the charge was very little diſſipated. I repeated this experiment, with the ſame event, with oil of olives, the thinneſt oil of turpentine, and even ether. A plate of common oil of olives connected the inſide and outſide of the battery for near ten minutes, without my being able to perceive that the charge was more diſſipated than it would have been without that communication. Ether is the ſlighteſt fluid in nature next to air; yet, being properly an oil, it proved no better a conductor than the moſt tenacious. I was moſt ſurpriſed that the ether did not take fire by this treatment, as nothing is more inflammable; and if the electric matter can paſs through it, nothing fires ſo ſoon.

FROM theſe experiments, and thoſe above mentioned, on ice, I concluded, that fluidity, as ſuch, contributes nothing to the conducting power of ſubſtances, ſeparate from the heat which makes them fluid. To complete my experiments on oils, I filled phials with all kinds of oils, according to their chymical diſtinctions, including the fineſt *eſſential oils*, the ſtrongly *empyreumatic*, and thoſe that are termed mineral, as *oil of amber*; and found them all incapable of giving a ſhock. But I found that this method of trying the conducting power of ſubſtances, viz. by incloſing them in phials, and endeavouring to give ſhocks by them is very inaccurate, ſhowing them to be better conductors than they really are. Pounded glaſs, flower of brimſtone, and other electric ſubſtances gave a conſiderable ſhock; but a bottle containing nothing but air gave a greater ſhock than any of them; though the wire inſerted into it was very blunt, and was kept in the center of the bottle. Finding, by theſe experiments, that oil plainly conducted much leſs than air, I endeavoured to charge a plate of oil like a plate of glaſs; and for this purpoſe I perforated a glaſs ſalver, and thereby gave a coating of tinfoil to both ſides of a quantity of oil poured into it; but the brim of the ſalver would not contain enough to give it a ſufficient thickneſs; otherwiſe, I make no doubt, but that a ſhock might be given by it better than by air.

I SHALL juſt mention upon this ſubject, what I lately obſerved, and do not know whether it has been noticed by any writer, that *ice of oil*, contrary to ice of water, is ſpecifically heavier than the fluid ſubſtance, and ſinks in it *.

FINDING ſo great an agreement, with reſpect to electric properties, in this whole chymical claſs of bodies, I began a kind of courſe of chymical electricity; but had not leiſure, or opportunity to purſue it as it deſerved. The few hints that I collected may poſſibly be of ſervice to future inquirers; and for this reaſon I ſhall note them juſt as they occurred, though they contain little that is remarkable.

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* OTHER perſons, I find, have made experiments, which ſhow how imperfect a conductor oil is. The following proofs of this are exceedingly curious and pleaſing. Mr. Cigna obſerved electrical attraction and repulſion between conducting ſubſtances plunged in oil. Nollet's Letters, Vol. iii. p. 168.

MONSIEUR VILLETTE, optician at Liege, filled a diſh of metal with oil, and when he had electrified the diſh, he plunged a needle into the oil, and received a very ſtrong ſpark as ſoon as the point of it came within a ſmall diſtance of the diſh. A ſmall cork ball being made to ſwim in this oil, upon the approach of the thicker end of the ſtalk of a lime, plunged to the bottom, and immediately roſe again to the top. Nollet's Letters, Vol. iii. p. 312.

All *ſaline ſubſtances* that I examined proved, in general, pretty good conductors. I tried moſt of them by making the diſcharge of the battery through them when inſulated; which appears to me to be a very good method, indeed the only one that can well be depended upon. In diſcharging the battery with a piece of *alum*, the exploſion was attended with a peculiar hiſſing noiſe, like that of a ſquib. *Rock ſalt* conducted pretty well, but not quite ſo well as the alum. The electric ſpark upon it was peculiarly red. *Sal ammoniac* exceeded them both in its conducting powers, but it would not take the leaſt ſenſible ſpark; ſo that it ſeemed made up of an infinite number of the fineſt points. *Volatile ſal ammoniac* I only tried in a phial, when it gave a ſmall ſhock. *Salt petre* did not conduct ſo well as ſal ammoniac. Endeavouring to make the electric exploſion paſs over its ſurface, it was diſperſed into a great number of fragments in all directions with conſiderable violence. *Selenitic ſalt* conducted a ſhock but poorly. *Vitriolated tartar* gave a ſmall ſhock. *White Sugar* ſeems to be an exception to this rule: for it may be fairly ſaid to be no conductor; as the charge of the battery would hardly paſs through it in the leaſt degree.

The *metallic ſalts* in general conducted better than other neutrals: *blue and green vitriol* conducted very well, though they would not tranſmit a ſhock.

That *ores* in which the metal is really in a metalline ſtate ſhould be very good conductors might naturally be expected. Thus a piece of gold ore from Mexico was hardly to be diſtinguiſhed, in this reſpect, from the metal itſelf; and a piece of ſilver ore from Potoſi, though mixed with pyrites, conducted very well. But even ores in which the metal is mineralized with ſulphur and arſenic, as the ores of lead and tin, and *cinnabar* the ore of quickſilver were little, if at all, inferior to them. The cinnabar that I tried was factitious; but there can be no doubt of its being the ſame as the native. When I made the exploſion of the battery paſs through it, it was rent into many pieces, and the fragments diſperſed in all directions. Ores, however, that contain nothing but the earth of the metal conduct electricity but little better than other ſtones; though I thought that all the ſpecimens of iron ore that I tried conducted better than marble *.

I EXAMINED ſome *black ſand* that came from the coaſt of Africa, which is a good iron, and part of which is affected by the magnet as much as ſteel filings; and found it to conduct electricity, but not a ſhock. Separating with a magnet all that would be eaſily attracted by it (about one ſixth of the whole) it conducted a ſhock very well. The reſt would hardly conduct at all.

THOUGH I think I may venture to ſay, that the true and proper ores of the more valuable metals might be known by their property of conducting electricity, I cannot ſay that electricity will furniſh any rule to aſcertain the value of the different ores of the ſame metal. I tried two pieces of copper ore, one the moſt valuable that is known, and another of only about half the value; but they were hardly to be diſtinguiſhed from one another in their conducting power.

BLACK *lead* in a pencil conducted a ſhock ſeemingly like metal or charcoal. A ſmall lump of it took as full and ſtrong a ſpark from the prime conductor as a braſs knob.

ALL the *ſtony ſubſtances* that I tried conducted very well, though dry and warm. Even a piece of poliſhed *agate*, though ſemi-pellucid, received the electric ſpark into its ſubſtance;

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* I FIND that Mr. Boze, very early, thought it was eaſy to diſtinguiſh the ores of metals from other earthy ſubſtances, by means of their greater conducting power. Dantzick Memoirs, Vol. i. p. 293.

though it would paſs over about three quarters of an inch of its ſurface to reach the finger that held it, and it diſcharged the battery but ſlowly. *Limeſtone*, and *lime* juſt burnt were equally imperfect conductors, hardly to be diſtinguiſhed from one another. *Lapis hæmatites*, and *touchſtone* both conducted pretty well; as did a piece of *gypſum*, and *plaiſter of Paris*, only the latter, having a ſmoother ſurface, took a ſtronger ſpark. A piece of *ſlate*, ſuch as is commonly uſed to write on, was a much better conductor than a piece of *free ſtone*, which conducted very poorly. *Marbles* alſo conducted conſiderably better than free ſtone. I found very little difference among any of the ſpecimens of marble that I tried, in which was a piece of Egyptian granite. A piece of Spaniſh chalk, which is a *talc*, conducted pretty much like marble.

A LARGE piece of white *ſpar*, with a tinge of blue, and ſemitranſparent, would hardly conduct in the leaſt degree. I took pretty ſtrong ſparks from the prime conductor while it was in contact with it.

A PIECE of *pyrites* of a black colour took ſparks at a conſiderable diſtance from the prime conductor, like ſome of the inferior pieces of charcoal. Another piece of pyrites, which had been part of a regular ſphere, conſiſting of a ſhining metallic matter, did not conduct near ſo well, though much better than any other ſtony ſubſtance. It was a kind of medium between a ſtone and an ore.

A PIECE of *aſbeſtos* from Scotland, juſt as it is taken from its bed, would not conduct. It was in contact with the conductor, while I took ſparks at the diſtance of half an inch with a moderate electrification.

OF *liquid ſubſtances*, *oil of vitriol* conducted pretty well, and the moſt highly rectified *ſpirit of wine* gave a ſhock much like water, but perhaps not quite ſo well.

This courſe of experiments on the conducting power of ſubſtances, according to their chymical claſſes, would, probably, be very uſeful, if purſued with care. Thoſe mentioned above were generally ſingle experiments, which are not ſo much to be depended upon.

There are ſome other *mixed ſubſtances* whoſe conducting power I have tried, and becauſe I think it would not be eaſy to ſay, *a priori*, to which of the two claſſes they belong, I ſhall juſt mention the reſult of my experiments upon them, nearly in the order in which they were made.

Dry *glue*, which is an animal ſubſtance, is a conductor of electricity, but does not conduct a ſhock.

Pounded *glaſs* mixed with the white of an egg, and which had ſtood till it was perfectly dry, was a conductor. I had put it upon ſome broken jars, thinking that the compoſition would be an electric ſubſtance, and that it would make the jars hold a charge again.

Paint, made of white lead and oil, very old, and dry, proved a conductor. I tried it in a china veſſel which had been firmly pieced with it. A part of the veſſel, through which there was no crack, would receive a charge very well; but a piece in which there was a crack, and which had been filled with this cement, could not be charged at all.

SECTION V

EXPERIMENTS ON THE DIFFUSION OF ELECTRICITY OVER THE SURFACES OF GLASS TUBES, CONTAINING A NEW METHOD OF GIVING THE ELECTRIC SHOCK.

IT had been obſerved by many electricians, that new globes are often difficult to excite; but I have made ſome experiments, which prove this fact, and other differences between new and old glaſs, in a more diſtinct manner than any thing elſe I have yet met with; but they leave the cauſe ſtill unexplained.

THE moſt remarkable property of new flint glaſs is the eaſy diffuſion of electricity over its ſurface. I have ſeveral times got tubes made two or three yards long, terminating in ſolid rods. Theſe I have taken almoſt warm from the furnace, in the fineſt weather poſſible, and having immediately inſulated them, and hanging pith balls at one extremity, have always found, that they would ſeparate the moment that the wire of a charged phial was applied to the other end. This I had reaſon to think would be the caſe at almoſt any diſtance at which the experiment could be made. I have even charged a phial very ſenſibly, when it was held cloſe to the glaſs, at the diſtance of a yard from the wire of a charged phial, held cloſe to another part of it, the coatings of both phials being held in my hands. When the ſame tubes were a few months older, I found that the electric virtue could not be diffuſed along their ſurfaces farther than about half a yard.

SOME tubes, which I have tried the day they were made, I have found impoſſible to be excited in the leaſt degree, even with the uſe of oiled ſilk and amalgam, for an hour together; when a ſingle ſtroke of the ſame rubber has rendered other tubes highly electrical, and two or three have made them to emit ſpontaneous pencils. The ſame new tubes, upon being much rubbed, have begun to be excited, and in a few days have acted pretty well.

BUT that the firſt coat of new glaſs is, in ſome meaſure, a conductor of electricity, was moſt evident from ſome experiments which I made with long and very thin tubes, which were blown ſome time in the month of March. Theſe, to amuſe myſelf, I coated in different places, and the diffuſion of electricity, from the coated part to that which was not coated, appeared to me very extraordinary. I think my reader will not be diſpleaſed if I relate a few of the particulars.

I PROCURED a tube, open at both ends, about a yard in length, but of very unequal width. About three inches of the middle part of it I coated on both ſides; and charging it, by means of a wire introduced at one of the ends; I perceived, not only that the part through which I had introduced the wire was ſtrongly electrical on the outſide, but that at the oppoſite end, where there was neither coating nor wire, the fire crackled under my fingers, as I drew the tube through them, and a flame ſeemed to iſſue continually out at both the ends, while it was at reſt and charged. N. B. One end of this tube was broken, and rough, the other was ſmooth.

I procured another tube, about an inch in diameter, and very thin. It was about three feet and a half in length, and cloſed at one end. About nine inches below the mouth, I coated three inches of it, both on the inſide and outſide. This part I charged, and then obſerved the whole tube, to the very extremity of it, to be ſtrongly electrical, crackling very loud when I drew my hand along it, and giving ſparks, as from an excited tube, at about the diſtance of an inch, all the way.

To give the reader a better idea of theſe experiments, I have given a drawing [Pl. I. fig. 7] of one of the tubes with which they were made. It is open at one end, and the part [*a*] is coated.

After drawing the whole tube through my hand, all the electricity on the outſide was diſcharged; but, upon putting my finger within the mouth of the tube, an effort to diſcharge itſelf ſeemed to be produced, which ſhowed itſelf by a light ſtreaming viſibly from the coating, both towards the finger and likewiſe as vigorouſly towards the oppoſite end of the tube. After this I found all the outſide of the tube loaded with electricity as before, which might be taken off, and revived again many times, with the ſame original charge; only it was weaker every time.

Holding this tube by the coated part, and preſenting the uncoated outſide, near the cloſe end of the tube, to the prime conductor, the inſide became charged as well as the outſide; and, upon introducing a wire, a conſiderable exploſion was made.

The diſcharge made the outſide ſtrongly electrical, and by taking this electricity off, the tube was charged again very ſenſibly.

Holding it by the uncoated part, and preſenting the coated part to the conductor, the inſide became charged as before.

HAVING firſt perfectly diſcharged this tube, I cloſed the open end with cement, made of bees wax and turpentine, an inch or more in thickneſs; but ſtill, by applying the outſide of the tube (either the coated or the uncoated part) to the conductor, I found it manifeſtly charged, but not quite ſo high as when the end was left open, though the difference was not great.

I PROVIDED myſelf with another tube, about an inch and a quarter wide, and three feet long; but it was drawn out one foot more very ſmall; and another foot at the extremity was ſolid, ſo that it was in all five feet long, I coated about four inches of this tube, two feet below the mouth of it. The balls being hung at the extremity of this tube, or rather of the ſolid rod in which it terminated, they ſeparated the moment I began to charge the coated part. The diſcharge brought them together, though not immediately, but a ſecond diſcharge would generally do it.

THE reſiduums of any of theſe tubes, of which ſo ſmall a part was coated, were very conſiderable. I thought that all of them might be equal to the firſt diſcharge. In the laſt mentioned tube, there was a reſiduum after a great number of diſcharges, I believe twenty or thirty.

IMAGINING that the diffuſion on the ſurfaces of the tubes above mentioned depended upon the *newneſs* of the glaſs, I preſerved them ſix or ſeven months; having obſerved by examining them at proper intervals in the mean time, that this property, and others depending upon it, gradually leſſened; and before this time it was quite gone. There was no diffuſion of electricity over their ſurfaces, and they were as eaſily excited as other tubes, at the ſame time that they received a very good charge.

AT length, by ſome accident or other, all the tubes on which I had made theſe experiments were broken, except one, which was cloſed at one end, and which indeed, was the moſt remarkable of them all. Upon this tube, in the month of November, I began to renew my experiments, comparing it with others which I got made at that time, in order to aſcertain on what circumſtances this diffuſion of electricity depended. Theſe I ſhall diſtinctly relate, noting the time when each experiment was made, and every other circumſtance which I can imagine could poſſibly have any influence in the caſe.

November the 13th, I once more endeavoured to repeat the experiments above mentioned with the old thin tube, with as much care and precaution as poſſible, but without the leaſt ſucceſs. At the ſame time I charged two other thin tubes, one cloſed, and the other open, after they had been made about ſix weeks, but without being uſed in the mean time, and they anſwered exactly as the former tube had done, when it was new. The charge from a ſmall coated part diffuſed itſelf all over the tube; ſo that at the diſtance of a yard from the coating, it gave ſparks to the finger of an inch in length, and in all reſpects exhibited the appearance of a tube freſh excited. On this occaſion I firſt obſerved, what afterwards drew my attention in a more particular manner, that when my finger was brought to the tube about two inches above the coating [as at *b*. Pl. I. fig. 7.] it diſcharged a great quantity of that diffuſed electricity; and my whole arm was violently ſhocked.

November the 19th. After heating the old tube, and endeavouring to repeat the former experiments, both while very warm, and after it was cold again, but to as little purpoſe as before; I took it to the glaſs-houſe, and got it made red-hot all over, ſo that it would eaſily bend any way; and as ſoon as it was cold, I tried the old experiments, and found that it had completely recovered its former property. Charging a ſmall coated part, the electricity was diffuſed to the end of the tube, over three feet of dry glaſs, and it gave ſparks at the diſtance of an inch in any part of it, exactly as if it had been excited with the beſt rubber. When it was drawn through my hand, whereby that diffuſion was taken off, it preſently returned again; and the extremity of the tube would get loaded while its communication with the coating had been cut off, by my hand being conſtantly held on the middle of it.

I ALSO obſerved, that the middle part of this tube, which had been ofteneſt heated, in melting the whole over again, one half at a time, had a much ſtronger diffuſion than the other parts. It was no ſooner taken off, than it appeared again, ſo that it gave a continual ſtream of fire.

THE quantity of *reſiduum* after a diſcharge of this tube was prodigious, ſo that the outſide coating would, immediately after, give almoſt a conſtant ſtream of fire to any conductor preſented to it, for a conſiderable time.

THIS tube was now, as it had been at the firſt, abſolutely incapable of being excited with the beſt rubber.

JANUARY the 6th, 1767. Examining all the tubes with which I had made the experiments of the diffuſion, I found that property either quite, or very nearly gone. One of them I reſtored by heating it red-hot. Another I heated only at the end moſt remote from the coating; but there was no diffuſion upon it, when the coated part was charged; the part which had not been made red-hot intercepting it.

NOVEMBER the 24th. In order to determine whether this property of diffuſion depended in any meaſure upon the ſmoothneſs of the ſurface, I made a circular part of one of the thin tubes, about half a yard beyond the coated part, quite rough with emery, about three inches in length; but this did not prevent the diffuſion in the leaſt; both that rough part, and the ſmooth glaſs beyond it were as much loaded with the electricity as the reſt.

I then took the poliſh off a line the whole length of the tube, from the coating to the extremity of it; but ſtill the effect was the ſame: and I make no doubt would have been ſo if I had made all the ſurface rough.

In order to aſcertain whether this property depended upon the thinneſs of the tubes, I got one made of a twelfth of an inch thick, and uſed it immediately; the diffuſion was very ſenſible, and it was incapable of being excited. This, however, was not always the caſe with tubes of ſo great a thickneſs.

November the 25th. Willing to carry this experiment a little farther, I got another tube four feet long, and of the eighth of an inch thick. I coated a ſmall part of it in the ſame manner as I had done the others, namely about three inches, at the diſtance of nine inches below the orifice, and obſerved the diffuſion to be very remarkable in proportion to the charge it received, which was very moderate. It could not be ſenſibly excited in the leaſt degree; except that, in the dark, an exceedingly ſmall light was viſible near the finger, when it touched any part of it, immediately after excitation; but not the leaſt ſnapping could be perceived, nor any thing felt with the finger.

To find whether this property depended upon the *kind*, as well as the newneſs of the glaſs, I afterwards, coated a part of a very thin glaſs of the common bottle metal, but I found no diffuſion upon it at all. It was what is commonly called a ſinging glaſs. I would have purſued this experiment by trying the ſame glaſs in other forms, and by trying other kinds of glaſs, but I had no opportunity.

I observed, in all the tubes which had the diffuſion, that in drawing my hand from the extremity of them towards the coating, after they were charged, ſo as to take off the diffuſion, there was a conſiderable noiſe at the orifice, as if the tube had been gradually diſcharging itſelf, and this operation did apparently leſſen the charge.

In the dark the electric fire ſeemed to pour perpetually from the open end, or both the ends, if they were both open; and whenever I drew my hand over it, the fire ſtreamed from the coating towards my hand in a very beautiful manner.

It was very remarkable, that, the firſt time I charged any of theſe tubes after they had ſtood a while, the diffuſion was the moſt conſiderable, and that it leſſened every ſucceſſive charge; till, at laſt, it was exceedingly ſmall; but after the tube had ſtood a few hours uncharged, it was as vigorous as ever.

December the 1ſt. I, for the firſt time, took particular notice in charging a thin tube, and afterwards holding the coating in one hand, and drawing my other hand, ſo as to graſp the tube; beginning at that end which was moſt remote from the coating; that ſometimes, when my hand came near the coating [as at *b*. Pl. I. fig. 7.] I received a very conſiderable ſhock through both my arms and in my breaſt, exactly like what is felt from the Leyden phial.

The ſame day, I felt a ſimilar ſhock from another thin tube; and what was more remarkable, I did not receive it till the third time of drawing my hand over the tube, having miſſed the ſtroke the two firſt times; though I moved my hand, as near as I could judge, in the ſame manner. This ſhock was not very great, but ſenſible in both arms.

December the 3d. I received another ſhock, the third time of drawing my hand over the tube, and much more violent than the laſt; it affecting both my arms and breaſt. At this time I obſerved very exactly, that my hand was near two inches and an half from the coating, and that a ſtrong light was viſible under my hand, and extended to the coating. The diffuſion at this time had not been very great, and the tube ſeemed to be about half diſcharged after the ſhock.

At that time I could not think of any plauſible theory to account for this ſhock; but preſently after I accidentally received another ſhock, in ſome reſpects ſimilar to this, the theory of which I have been ſo happy as to inveſtigate, and which may throw ſome light upon this.

December the 21ſt. I made a Torricellian vacuum in a tube about a yard in length [Pl. I. fig. 8.] and holding one end of it in my hand, I preſented a part near the other end to the prime conductor; and obſerved, that, while the electric fire was pouring along the whole length of it, I felt ſome peculiarly ſmart twitches every now and then in my hand, juſt ſuch as are felt when a thin uncoated phial is held in the hand, while it is charged at the prime conductor, but more pungent. On removing the tube from the prime conductor, it threw out ſpontaneous ſparks from the place where it had touched the conductor, exactly like thoſe which iſſue from the wire of an overcharged phial; but they were longer, and much more beautiful. Then, bringing my other hand near the place where the tube had touched the conductor, I received a very conſiderable ſhock in both my arms and breaſt, exactly like that which I had received before from the thin tubes; and, as with them, the ſhock was rather ſtronger in the hand which was brought to the tube than in that which held it. If, without bringing my other hand to the tube, I only preſented it to the table, or any other conductor, it would throw out from the ſame place ſeveral ſtrong ſparks, attended with a flaſh of light, which filled the whole length of the tube. Theſe ſparks reſembled thoſe which iſſue from the wire of a charged phial, when it is preſented to the like imperfect conductors, and at the ſame time held in the hand.

I AFTERWARDS obſerved, that the ſtrongeſt ſhock which this tube could give was felt when one hand continued in the place where it held the tube, in order to charge it, and the other was made to touch the tube, an inch or two above it [as at *c*. Pl. I. fig. 8.] and at the inſtant of the ſtroke a very denſe ſpark of electric fire was ſeen darting the whole length of the tube. When three perſons beſides myſelf joined hands, it ſhook all our arms greatly.

THE tube could not be diſcharged by putting one hand ſo near the other, unleſs that part of the tube had been brought to the conductor in charging it; and if any particular part of the tube only, had been brought to the conductor, the diſcharge could not be made without touching that part.

WHEN the tube had given a ſhock from any one place, it would give one or two more ſmaller ſhocks from other places.

THE experiments I made with this tube being certain and invariable, and the ſhock I received from the other tubes precarious, I gave more particular attention to this, in order to aſcertain the nature of this ſhock; thinking that, if I could accompliſh this, it would aſſiſt me in the inveſtigation of the other. Accordingly, I coated about ſix inches, near each end of the tube, [*a* and *b* Pl. I. fig. 8.] leaving the ſpace of about half a yard of uncoated glaſs between the coatings; and obſerved, that when I held one of theſe coatings in my hand, and preſented the other to the prime conductor, it always received a conſiderable charge, and was diſcharged in one bright ſpark at the diſtance of above an inch, and ſometimes two inches, if, beſides the coated part, I had likewiſe preſented the uncoated part to the prime conductor; and ſometimes the uncoated part would diſcharge itſelf by a bright flaſh to the lower coating, leaving the coated part charged as before. If I held the tube by the middle, where there was no coating, and preſented one of the coatings to the conductor, it received a pretty good charge.

I THEN ſtood upon an inſulated ſtool, and preſenting one of the coatings to be charged, while I held the other; I obſerved, that it received not more than one fourth part of the charge it had before; upon which I immediately concluded, that the lower coating muſt have been charged negatively, whilſt the upper was charged poſitively. This was quite confirmed by obſerving, that ſparks could be drawn from my body, while I ſtood upon the ſtool preſenting the tube to be charged, but no longer than till the tube had received its full charge; and that then the exploſion was as great as it had been when I ſtood upon the floor.

WHEN I inſulated the tube, by placing it in a glaſs veſſel, it was ſtill leſs capable of taking a charge than when I ſtood upon a ſtool and held it, this method making a more perfect inſulation. If any conductor was preſented to the lower coating while the other was held to the prime conductor, ſparks iſſued from it very plentifully till it had got a conſiderable charge; when thoſe ſparks intirely ceaſed, and the tube, upon trial, gave a very great exploſion.

THESE experiments make the theory of this new method of giving the electric ſhock pretty obvious. The electric matter thrown upon the upper coating repels an equal quantity from the inſide of the tube oppoſite to it; which, paſſing freely through the vacuum (as is viſible in the dark), is accumulated on the inſide of the other extremity of the tube, and thereby repels a quantity from the lower coating: ſo that the two coatings being in oppoſite ſtates, though on the ſame ſide of the tube, the ſame kind of ſhock is given by them, as if they had been on oppoſite ſides.

BEING fully ſatisfied with the experiments made by theſe two coatings, and the theory of them, I amuſed myſelf with coating the middle part of the tube in various ways.

WHEN the three coatings were about the ſame ſize, and placed at equal intervals; which ever of them was held in the hand, the other two were charged and diſcharged ſeparately. If the coating of one of the ends was held in the hand, and the other two were charged, the greateſt exploſion was from that which was diſcharged firſt. If thoſe two coatings were placed near one another, they were both diſcharged by the attempt to diſcharge either of them, and a flaſh of light was ſeen betwixt them both. In this caſe the exploſion was ſometimes made at the diſtance of two inches and a half.

WHEN the middle coating was made very large, and placed contiguous to the upper, the exploſion was leſs; a ſpontaneous diſcharge being ſoon made to the lower coating.

WHEN the middle coating was taken away, it often happened that, in drawing the whole tube over the prime conductor, beginning at the upper coating; when it came to the lower, by which I held it, a ſpark would dart to it from all the uncoated part of the tube, which diſcharged the electricity of that part, while the upper coating ſtill retained its proper charge.

WHEN this ſpontaneous diſcharge was not made, the exploſion might be made at twice, once at the naked glaſs, near the lower coating, and again at the upper coating. If the diſcharge was firſt made at the upper coating, there remained very little for the lower part of the tube. And if the exploſion was made about the middle of the tube, the whole was diſcharged at once, and in a very beautiful manner.

I MUST leave my reader to compare the theory of this ſhock with that given by the long and open tubes, as I am not able to do it to my intire ſatisfaction without more experiments;

which, as I obſerved, are precarious, and which I had not leiſure to attend to.

As this courſe of experiments was begun by an accidental obſervation of the different electric properties of new and old glaſs, I ſhall (after this long excurſion, which I little foreſaw) conclude with an experiment or two relating more immediately to the original ſubject of them.

IMAGINING that the difference between new and old glaſs might be owing to the larger ſuperficial pores of the former, which made it approach to the nature of a conductor, and which contracted with time; I thought it might poſſibly be determined by the experiment of the metallic tinge, the wider pores receiving it better than the ſmaller; and I was not diſappointed in my expectations. November the 19th, I ſeveral times laid two glaſs tubes, one a very old one, and the other quite new, cloſe together, with a piece of leaf gold or copper between them; and though I varied the diſpoſition of them in every way that I could think of, and changed the tubes for others; I always found the new glaſs to receive a much fairer, more beautiful, and indelible impreſſion than the old glaſs. Twice the quantity of the metal was in all the caſes ſtruck into it.

SECTION VI.

EXPERIMENTS TO VERIFY SEVERAL PARTICULARS OF SIGNIOR BECCARIA'S THEORY OF ELECTRICITY; PARTICULARLLY CONCERNING THE ELECTRIC MATTER CARRYING INTO ITS PATH LIGHT SUBTANCES TO ASSIST ITS PASSAGE.

BEING greatly ſtruck with Signior Beccaria's theory concerning the paſſage of the electric matter from the earth to the clouds, previous to a thunder ſtorm, and thinking his experiments to prove the power of electricity to conduct into its path light ſubſtances that could aſſiſt its paſſage not quite ſatisfactory, I endeavoured to aſcertain the fact in a better manner, and ſhall lay before my readers the reſult of my experiments.

NOVEMBER the 9th. I diſcharged frequent ſhocks, both of a common jar, and another of three ſquare feet, through trains of braſs duſt, laid on a ſtool of baked wood, making interruptions in various parts of the train; and always found the braſs duſt ſcattered in the intervals, ſo as to connect the two disjoined ends of the train; but then it was likewiſe ſcattered nearly as much from almoſt all other parts of the train, and in all directions. The ſcattering from the train itſelf was probably occaſioned by ſmall electric ſparks between the particles of the duſt; which, cauſing a vacuum in the air, drove all that light matter to a conſiderable diſtance. But the particles of the duſt which were ſtrowed in the intervals of the train, ſome of which were, at leaſt, three inches, could hardly be conveyed in that manner.

When ſmall trains were laid, the diſperſion was the moſt conſiderable, and a light was very viſible in the dark, illuminating the whole circuit. It made no difference, in any of theſe experiments, which way the ſhock was diſcharged.

When I laid a conſiderable quantity of the duſt at the ends of two pieces of chain, through which the ſhock paſſed, at the diſtance of about three inches from one another, the duſt was always diſperſed over the whole interval, but chiefly laterally; ſo that the greateſt quantity of it lay in arches, extending both ways, and leaving very little of it in the middle of the path. It is probable, that the electric power would have ſpread it equably, but that the vacuum made in the air, by the paſſage of the fluid from one heap of duſt to the other, diſperſed it from the middle part.

I then inſulated a jar of three ſquare feet, and upon an adjoining glaſs ſtand laid a heap of braſs duſt and at the diſtance of ſeven or eight inches a braſs rod communicating with the outſide of the jar. Upon bringing another rod, communicating with the inſide, upon the heap of duſt, it was diſperſed in a beautiful manner, but not one way more than another. However, it preſently reached the rod communicating with the outſide.

Making two heaps, about eight inches aſunder, I brought one rod communicating with the inſide upon one of them, and another rod communicating with the outſide upon the other. Both the heaps were diſperſed in all directions, and ſoon met; preſently after which the jar was diſcharged, by means of this diſperſed duſt, in one full exploſion. When the two heaps were too far aſunder to promote a full diſcharge at once, a gradual diſcharge was made through the ſcattered particles of the duſt.

WHEN one heap of duſt was laid in the center of the ſtand, and the two rods were made to approach on each ſide of it, they each attracted the duſt from the ſide of the heap next to them, and repelled it again in all directions. When they came very near the heap, the diſcharge was made through it, without giving it any particular motion.

ALL theſe experiments ſhow that light bodies, poſſeſſed of a conſiderable ſhare of electricity, diſperſe in all directions, carrying the electric matter to places not abounding with it; and that they ſometimes promote a ſudden diſcharge of great quantities of that matter from places where it was lodged, to places where there was a defect of it. But an accident led me to a much more beautiful, and perhaps a more ſatisfactory manner of demonſtrating the laſt part of this propoſition, than any that I hit upon while I was purſuing my experiments with that deſign.

DECEMBER the 11th. Hanging a drop of water upon the knob of a braſs rod communicating with the inſide of my battery, in order to obſerve what variety it might occaſion in the circular ſpots, which will be mentioned hereafter; I was greatly ſurpriſed to find the exploſion made all at once, at the diſtance of two inches.

I, AFTERWARDS, put ſome braſs duſt upon a plate of metal communicating with the inſide of the battery, and making the diſcharge through the duſt, it exploded at the diſtance of an inch and a half. The duſt roſe towards the diſcharging rod, and from thence was diſperſed in all directions.

THESE experiments are the more remarkable, as they demonſtrate ſo great a difference between the diſtance at which the battery may be made to diſcharge at once, by the help of theſe light bodies, and without them. When the diſcharge of a battery by the knobs of braſs rods, in the open air, is at the diſtance of about half an inch; it will, by this means, be made at about two inches.

Unless a perſon try the following experiment, he will hardly conceive the extreme probability of the clouds and the rain being poſſeſſed of an electric virtue, in order to their uniform diſperſion, according to Signior Beccaria's theory. Put a quantity of braſs duſt into a coated jar, and when it is charged, invert it, and throw ſome of the duſt out. It is very pleaſing to ſee with what exact uniformity it will be ſpread over any flat ſurface, and fall juſt like rain or ſnow. In no other method can it be ſpread ſo equably.

It is taken for granted by Signior Beccaria and others, that perſons are ſometimes killed by lightning without being really touched by it, a vacuum of air only being ſuddenly made near them, and the air ruſhing out of their lungs to ſupply it; and with ſo much violence, that they could never recover their breath. As a proof of this, he ſays, that the lungs of ſuch perſons are found flaccid; whereas, when they are properly killed by the electric ſhock, the lungs are found inflated. This account always appeared to me highly improbable. It determined me, however, to make a few experiments, in order, if poſſible, to aſcertain the fact with ſome degree of exactneſs. The reſult was as follows.

December the 18th. I placed that part of an egg ſhell in which is a bladder of air within an inch of the place where I made the exploſion of the battery, on the ſurface of ſome quickſilver; when the bladder was inſtantly burſt, and the greateſt part of it torn quite away. The ſhell was quite dry; ſo that the bladder could not ſtretch in the leaſt.

It is evident from this experiment, that there is a ſenſible expanſion of the neighbouring air, to fill a vacuum made by the electric exploſion; but that this is ſo conſiderable as to occaſion the ſuffocation and death of any animal, is, I think, very improbable from the following facts.

I put a cork as ſlightly as poſſible in the mouth of a ſmall phial; but, though I held it exceedingly near the place of the exploſion, it was not drawn out.

I made the exploſion paſs over the ſurface of a moiſt bladder, ſtretched on the mouth of a galley-pot; but it produced no ſenſible effect upon it. I alſo held at one time the bill of a robin red-breaſt, and at another time the noſe of a mouſe near the electric exploſion, but they did not ſeem to be at all affected by it. In order to examine the ſtate of the lungs, I killed ſmall animals by ſhocks diſcharged both through the brain, and through the lungs; but when they were diſſected there appeared no difference. The lungs were in the very ſame ſtate as when they were killed in another manner.

To theſe miſcellaneous experiments, intended to verify ſeveral particulars of Signior Beccaria's theory of electricity, I ſhall add a ſmall ſet, which, though they were begun before I had ſeen that author, are in ſome reſpects ſimilar to his curious experiment of diſcharging a plate of glaſs hanging by a ſilken ſtring without giving motion to it; his being deſigned to aſcertain the effect of the diſcharge upon the glaſs, and mine reſpecting the conducting ſubſtances that formed the circuit.

October the 7th. To determine, if poſſible, the direction of the electric fluid in an electric exploſion, I hung ſeveral braſs balls by ſilken ſtrings, and diſcharged ſhocks through them, when they were as much at reſt as I could make them; but I could not perceive that any motion was given to them by the ſtroke. Afterwards, I diſcharged a jar a great number of times through ſmall globules of quickſilver, laid on a ſmooth piece of glaſs; but could not perceive that they were driven one way more than another, though they were often thrown into diſorder; probably by the repulſion of the air, occaſioned by the vacuum of the exploſion.

I then placed four cork or pith balls, at equal diſtances, upon a ſtool of baked wood, with a piece of chain at the ſame diſtance from the outermoſt balls; and obſerved, that, upon every attempt to make a diſcharge, the two middle balls were driven cloſe together, while the two outermoſt were each of them attracted to the piece of chain that was next to it. Then, laying a great number of bits of threads in the ſame manner upon the ſtool, ſeveral of the pieces that lay near the chain ſtuck to them, and a great number of thoſe that lay in the middle were driven together in a heap.

The attraction to the chains I attribute to the electricity given to them by their connection with the jar, which would be greatly encreaſed by the attempt to make an exploſion; and the crowding together of the pieces in the middle of the circuit, I attribute to the current of air blowing them together from both the extremities of the chain. Thus part of the flame of a candle next to an electrified point will be attracted by the power of electricity, while the reſt of the flame will be repelled from it by the current of air.

These experiments led me to make a diſcharge through an inſulated bell, in order to obſerve in what manner it would be affected by the electric ſhock only, when it was not touched by any thing elſe. Accordingly, I made the diſcharge of the battery through it ſeveral times; and by each exploſion it was made to ring, as loud as it could be made to do with a pretty ſmart ſtroke of one's finger nail.

I ALSO made a diſcharge of the battery through the external coating of a glaſs jar, but without touching it with the diſcharging rods; and it plainly produced the ſame tone, as when it was rung by percuſſion.

SECTION VII.

VARIOUS EXPERIMENTS RELATING TO CHARGING AND DISCHARGING GLASS JARS AND BATTERIES.

AS ſeveral things have occurred, in the courſe of my experiments, relating to charging and diſcharging both common jars and large electrical batteries, which I have not ſeen in the writings of any electricians; and as ſome of the facts are not eaſily accounted for, I ſhall mention a few of the more remarkable of them, juſt as they happened.

APRIL the 28th. As I was amuſing myſelf with charging three jars of the ordinary ſize, while they ſtood upon a metal plate on the table, with their wires at different diſtances from the ſame prime conductor, which was fixed on pillars of baked wood; I obſerved, that whenever one of the jars, which ſtood next to the conductor, diſcharged itſelf, the others would diſcharge themſelves too; though they were far from having received their full charge, being placed at a greater diſtance from the common conductor, and conſequently having taken but few ſparks, in compariſon of that which ſtood the neareſt.

A VARIETY of experiments ſeem to ſhow, that, while a jar continues charged, the electric matter is continually inſinuating itſelf farther and farther into the ſubſtance of the glaſs, ſo that the hazard of its burſting is the greateſt ſome time after the charging is over.

MAY the 26th. After having charged forty-one jars together, each containing about a ſquare foot of coated glaſs, I let them ſtand about a minute and a half, while I was adjuſting ſome part of the apparatus, in order to make the diſcharge; when they exploded by the burſting of one of the number. I obſerved alſo, that the jar which was burſt was at a conſiderable diſtance from the place where I ſaw the flaſh at the wires. It was alſo broke through in two different places.

FOR the ſame reaſon, there is no being ſure that jars, which have ſtood one diſcharge, will bear another equally high. I am confident that ſeveral of mine have burſt with a much leſs charge than they had actually held before.

JUNE the 29th. A jar of an ordinary ſize, which had been in conſtant uſe for ſeveral months, and which had diſcharged itſelf more than a hundred times without any injury, at length burſt, as I was diſcharging it at the prime conductor. The hole was at a different place from that at which the diſcharge was made, but this does not always happen. The tip of my little finger happened to lie very lightly on the place, and I felt it was burſt by a ſmall pricking, as of a pin, though the exploſion at the conductor was nearly equal to that of any other diſcharge. The coating of a jar contiguous to one that is burſt is always melted by the exploſion.

JUNE the 25th. A ſmall thin phial, which had been charged ſingly as high as it could bear, ſo as to have diſcharged itſelf, and alſo in conjunction with four others of its own ſize, burſt by a ſpontaneous exploſion, when it was charged in conjunction with a battery.

I HAD never heard of a jar burſting in more than one place, or more than one jar in a battery burſting at the ſame time;

but I have often found, to my coſt, that this event is very poſſible. In this caſe, there muſt be a diſcharge at more places than one at the ſame time: and, beſides, it ſeems to follow from it, that whenever there is a ſolicitation, as we may ſay, to diſcharge at one place, the effort to diſcharge at every other place is encreaſed at the ſame time.

It has frequently happened with me, that jars have been burſt at the inſtant that I was making the diſcharge in the common way; and when I have come to charge them again, they have appeared to be burſt, in ſome place of the battery where I never expected it. Two inſtances of this kind happened in the exploſion mentioned above; but the moſt remarkable fact of this kind happened the 31ſt of May, when a battery of about forty jars, each containing a ſquare foot of coated glaſs, diſcharged itſelf.

Upon examination, I found that ſix of the jars were burſt, one had the tinfoil coating on the outſide quite melted, in a circular ſpot about half an inch in diameter; and in the inſide it was burned quite black, near an inch and an half. A ſecond was melted on the outſide, about three quarters of an inch in diameter, and the black ſpot in the inſide was two inches. A third had one hole made in the form of a ſtar, more ſmall cracks like *radii* proceeded from a center than could be counted. And there was hardly one of the jars that was burſt with a ſingle hole. Some were burſt in ſeven or eight different places, of which ſome were very remote from others; but generally there was one principal hole, and ſeveral ſmaller, but independent ones, in the neighbourhood of it, as within half an inch, an inch, or two inches from it.

June the 14th. The above mentioned battery diſcharged itſelf once more; when three jars were burſt, and one of them, beſides its principal hole, had a circular row of fractures, quite round the hole, at the diſtance of about half an inch. This appearance ſtruck me as ſomething very remarkable, but ſome light may perhaps be thrown upon it by a ſubſequent courſe of experiments. Each of the ſmaller fractures was about a tenth of an inch in length.

NOVEMBER the 17th. Having charged both my batteries, one of them, at that time, of thirty-one ſquare feet, and the other of thirty-two, I made the exploſion; when one jar in each battery was afterwards found to be broken. They broke at the inſtant of the diſcharge, ſo that I did not ſuſpect what had happened. Both the batteries had frequently borne a much higher charge. In one of the ſmaller jars, the coating, beſides being burſt oppoſite to the hole, was rent about an inch and an half along a crack that was made from it.

WHEN jars diſpoſed in batteries have been burſt in this manner. I have never failed to obſerve one circumſtance which appears to me truly remarkable. It is, that though, in this caſe, ſeveral paſſages be opened for reſtoring the equilibrium of the electric fluid, yet the whole ſeems to paſs in the circuit that is formed for it externally. At leaſt, the effect of the exploſion is not ſenſibly diminiſhed, upon any ſubſtances that are expoſed to it. This I had a fair opportunity of obſerving when I was tranſmitting the exploſion of the battery through wires of different metals. I found that the utmoſt force of the battery would do little more than melt a piece of ſilver wire on which I was trying it, and yet it was, at one time, totally diſperſed by an exploſion, in making which three jars were broken in different parts of the battery.

THE moſt remarkable fracture I remember, was of a jar an eighth of an inch thick, and which was therefore, for a long time, thought to be too thick for uſe. This jar, however, which had never held but a moderate charge, burſt ſpontaneouſly;

when there was found in it one hole like what is commonly obſerved, from which extended two cracks that met on the oppoſite ſide of the jar, ſo that it came in two parts: but, beſides this, there were two other holes, barely viſible to the naked eye, at the diſtance of ſome inches from the principal hole, and conſiderably diſtant from one another. Yet theſe holes, when examined with a microſcope, were plainly fractures, like others made in the ſame manner, having a white ſpeck in the middle. One of them was above the external coating, but not above the internal.

I HAD frequently been much ſurpriſed at the great diſtance at which ſeveral of my jars would diſcharge themſelves, one of five inches being very common. This induced me to try at what diſtance I could make that ſpontaneous diſcharge.

FEBRUARY the 21ſt. I got a jar, eight inches and a half in depth, and three in diameter. Finding it diſcharged itſelf very eaſily, when coated in the uſual manner, that is, about four inches from the top; I cut the coating away, till I had brought it within two inches and a quarter from the bottom; when it ſtill retained the ſame property; and, at length, it burſt by a diſcharge through a white ſpeck of unvitrified matter, an inch and three quarters above the top of the coating.

I THEN procured a jar made blue with zaffre, ſeven inches and a half high, and two inches and an half in diameter. I coated of it only one inch and a quarter from the bottom, and yet it diſcharged itſelf very readily. I afterwards, by degrees, cut the coating down to little more than half an inch from the bottom, and ſtill the diſcharge continued to be made as before. This property it retained till the month of October following, when it was broke by an accident.

I HAVE another blue jar, of nearly the ſame ſize with the former, which is almoſt full of braſs duſt, but has no coating at all.

all on the outſide. Yet, if I ſet this jar upon the table, in contact with a ſingle piece of braſs chain, going quite round it, and lying upon the table, it will diſcharge itſelf the whole length of the glaſs. N. B. The manner in which the uncoated part of theſe jars becomes charged exhibits an exceedingly beautiful appearance, eſpecially in the dark; the fire flaſhing from the top of the coating, in the form of branches of trees, firſt on one ſide of the glaſs, and then on the other, and growing larger and larger till they go over the top.

I HAVE made ſome experiments, to try how thick a plate of glaſs may be charged, but I have not been able to aſcertain this circumſtance with any degree of exactneſs: I only found, that I was not able to give the leaſt charge to a plate of glaſs half an inch thick, when it was not warmed. It was the bottom of a large glaſs tumbler; but meeting with it only upon a journey at the houſe of an ingenious electrician, I had no opportunity of making many experiments upon it. I imagine that warming it would have made it capable of being charged. Glaſs of a quarter of an inch thick will hold a pretty good charge.

MR. KINNERSLEY's experiments leaving me no reaſon to doubt, but that Florence flaſks were capable of receiving a charge like any other thin glaſs, which might be made a conductor by heat, I imagined I could ſoon conſtruct a very ſtrong and very cheap electrical battery out of them. Accordingly I procured a few, for a ſpecimen, but was greatly ſurpriſed to find that the electricity went through them, when quite cold, like water through a ſieve, without making any fracture in them: for they continued to hold the ſame ſmall charge, which was different in different flaſks. Mentioning this diſappointment to Mr. Canton, he informed me, that he had met with the ſame, and that the permeability of this kind of glaſs to the electric fluid was owing to ſmall unvitrified parts which may be ſeen in them. I thought it might be of uſe to publiſh this fact, as it may prevent other perſons from being diſappointed in the ſame expectations.

As glaſs had generally been charged when it was ſmooth, and electrics which had the property of rough glaſs, when excited, were exceedingly difficult to charge; I had the curioſity to try what might be done with *rough glaſs* itſelf. Accordingly, I firſt made part of a jar rough, connecting the inſide with the outſide coating; thinking that the roughneſs might poſſibly promote a ſpontaneous diſcharge; but I found it was not made in that place preferably to any other. I afterwards took the poliſh from all that part of the outſide of a jar that was above the coating, but it was charged and diſcharged exactly as before. Laſtly, I made a plate of glaſs rough on both ſides, taking off all its poliſh, and found that it received a charge as well as a ſmooth plate.

The manner in which tubes and plates of glaſs have broken, when I have failed to ſtrike a metallic tinge into them, by the diſcharge of an electrical battery, have ſometimes been attended with circumſtances which I cannot eaſily account for. The following are the facts.

December the 3d. Endeavouring to fix a metallic tinge upon a flat piece of glaſs, it was broken by the exploſion, parallel to the line along which the metal lay, at about an inch diſtance, but not where the tinge itſelf was.

In attempting to give a metallic tinge to a part of a long glaſs tube, it broke, though not in the place where the tinge was made, but on the oppoſite ſide, which was ſhattered all to pieces. The leaf gold had been bound tight to the glaſs, under a piece of paſteboard, which covered the gold, but not all the tube. Another tube alſo was broken in large fragments where the metal had been put on, but into ſmall ſplinters on the oppoſite ſide; and for the ſpace of ſix or ſeven inches farther, it was not broken at all on the ſide of the metal, but very much on the other ſide.

AT another time, in attempting the ſame thing with another glaſs tube; the end of it, which was near a foot diſtant from the place where the metal was laid, and which was a little cracked in an oblique direction, broke off in a round piece.

As few experiments have been publiſhed about *melting wires*, and procuring globules of metal by electrical diſcharges, and as ſeveral things have occurred in my attempts that way, which perhaps have not occurred to other perſons, I ſhall mention a few of the moſt material circumſtances. They will, at leaſt, ſerve as a direction to thoſe who may be diſpoſed to attempt the ſame thing.

I HAD frequently attempted to procure thoſe beautiful *globules of metal*, ſome of which I had ſeen with Mr. Canton; and for that purpoſe had made the diſcharge through ſmall wires laid in the bottom of china bowls, &c. but always without ſucceſs. At length, I thought of incloſing the wires in ſmall tubes; and this expedient fully anſwered my purpoſe: for, November the 12th, diſcharging a battery of thirty-two ſquare feet through an iron wire incloſed in a ſmall glaſs tube, I found innumerable globules of the metal, of very different ſizes. The whole piece melted was about two inches. Breaking the glaſs tube, I found the inſide ſurface uniformly covered with thoſe globules, and a black duſt, both fixed into the glaſs; ſo that they could not be ſeparated, without tearing away part of the glaſs.

THINKING to avoid this inconvenience, I fixed the ſmall wire in the center of a glaſs tube, of, at leaſt, a quarter of an inch in diameter; but, upon the diſcharge, this tube, though much wider than the former, was uniformly covered with the globules, and the black duſt, which ſtuck very faſt to it, though the metal did not ſeem to have penetrated into the ſubſtance of the glaſs. When the tube was broken, I ſcraped off part of the black lining, and the part next the glaſs looked like a thin plate of the metal.

IMAGINING that the melted metal would not adhere ſo cloſely to a conducting ſubſtance, as it had to the glaſs, I incloſed the wire in a paper tube a quarter of an inch wide. Upon opening it after the diſcharge, it was found uniformly covered with that black duſt, and the ſtain was every where indelible. Sparks of fire had been ſeen three feet from the place of diſcharge, but no part of the metal could be found.

I THEN confined the wire cloſer, wrapping it tight in paper. Upon the diſcharge, a great number of ſparks were ſeen, for about a ſecond of time, a quarter of a yard from the paper, which was burned through in ſeveral places. Very few pieces could be found, but thoſe were pretty large and irregular I now found, that, in order to produce theſe globules, the charge muſt be moderate, that when the charge was very high, the whole ſubſtance of the wire was diſperſed in particles too ſmall to be found; and, on the other hand, when the charge was not ſufficient, the metal was melted into fragments too large to form themſelves into regular globules.

WITH the ſame battery I once melted a piece of iron wire one ſeventieth of an inch in diameter, when a piece of it was thrown quite acroſs the table, to the diſtance of about ſix feet; where it fell upon a bureau, then tumbled down to the ground, and continued glowing hot all the time. At other times, ſparks from melted iron have been thrown three yards, in oppoſite directions, from the place of the fuſion, and continued a ſenſible ſpace of time red-hot upon the floor.

AT another time I had a very fine opportunity of obſerving what part of the conductors which form an electric circuit are moſt affected by the exploſion: for, upon diſcharging a battery of fifty-one ſquare feet through an iron wire nine inches long, the whole of it was glowing hot and continued ſo for ſome ſeconds; the middle part growing cool firſt, while both the extremities were ſenſibly red. Upon examining it afterwards, both the extremities were found quite melted; an inch or two of the part next to them were exceedingly brittle, and crumbled into ſmall pieces upon being handled; while the middle part remained pretty firm, but had quite loſt its poliſh, ſo that it looked darker than before.

SECTION VIII.

EXPERIMENTS ON ANIMALS.

AS I have conſtructed an electrical battery of conſiderably greater force than any other that I have yet heard of, and as I have ſometimes expoſed animals to the ſhock of it, and have particularly attended to ſeveral circumſtances, which have been overlooked, or miſapprehended by others; it may not be improper to relate a few of the caſes, in which the facts were, in any reſpect, new, or worth notice.

JUNE the 4th. I killed a rat with the diſcharge of two jars, each containing three ſquare feet of coated glaſs. The animal died immediately, after being univerſally convulſed, at the inſtant of the ſtroke. After ſome time, it was carefully diſſected; but there was no internal injury perceived, particularly no extravaſation, either in the abdomen, thorax, or brain.

JUNE the 19th. I killed a pretty large kitten with the diſcharge of a battery of thirty-three ſquare feet; but no other effect was obſerved, except that a red ſpot was found on the pericranium, where the fire entered. I endeavoured to bring it to life, by diſtending the lungs, blowing with a quill into the trachea, but to no purpoſe. The heart beat a ſhort time after the ſtroke, but reſpiration ceaſed immediately.

JUNE the 21ſt. I killed a ſmall field-mouſe with the diſcharge of a battery of thirty-ſix ſquare feet, but no other effect was perceived, except that the hair of the forehead was ſinged, and in part torn off. There was no extravaſation any where, though the animal was ſo ſmall, and the force with which it was killed ſo great. This fact, and many others of a ſimilar nature, make me ſuſpect ſome miſtake, in caſes where larger animals are ſaid to have had all their blood veſſels burſt by a much inferior force.

IN all the accounts that I have met with of animals killed by the electric ſhock, the victims were either ſmall quadrupeds, or fowls; and they are all repreſented as killed ſo ſuddenly, that it could not be ſeen how they were affected previous to their expiration. In ſome of my experiments, the great force of my battery has afforded me a pretty fair opportunity of obſerving in what manner the animal ſyſtem is affected by the electric ſhock, the animals which I have expoſed to it being pretty large; ſo that a better judgment may be formed of their ſenſations, and conſequently of the immediate cauſe of their death, by external ſigns. I do not pretend to draw any concluſion myſelf from the following facts. I have only noted them as carefully as I could for the uſe of phyſicians and anatomiſts.

JUNE the 26th. I diſcharged a battery of thirty-eight ſquare feet of coated glaſs, through the head, and out at the tail of *a full grown cat*, three or four years old. At that inſtant, ſhe was violently convulſed all over. After a ſhort reſpite, there came on ſmaller convulſions, in various muſcles, particularly on the ſides; which terminated in a violent convulſive reſpiration, attended with a rattling in the throat. This continued five minutes, without any motion that could be called breathing, but was ſucceeded by an exceedingly quick reſpiration, which continued near half an hour. Towards the end of this time, ſhe was able to move her head, and fore feet, ſo as to puſh herſelf backwards on the floor; but ſhe was not able to move her hind feet in the leaſt, notwithſtanding the ſhock had not paſſed through them. While ſhe continued in this condition, I gave her a ſecond ſtroke, which was attended, as before, with the violent convulſion, the ſhort reſpite, and the convulſive reſpiration; in which, after continuing about a minute, ſhe died.

Being willing to try, for once, the effect of a much greater ſhock than that which killed the cat upon a large animal, I gave an exploſion of ſixty-two ſquare feet of coated glaſs to a dog of the ſize of a common cur. The moment he was ſtruck, which was on the head (but, not having a very good light, I could not tell exactly where) all his limbs were extended, he fell backwards, and lay without any motion, or ſign of life for about a minute. Then followed convulſions, but not very violent, in all his limbs; and after that a convulſive reſpiration, attended with a ſmall rattling in the throat. In about four minutes from the time that he was ſtruck, he was able to move, though he did not offer to walk till about half an hour after; in all which time, he kept diſcharging a great quantity of ſaliva; and there was alſo a great flux of rheum from his eyes, on which he kept putting his feet; though in other reſpects he lay perfectly liſtleſs. He never opened his eyes all the evening in which he was ſtruck, and the next morning he appeared to be quite blind, though ſeemingly well in every other reſpect.

Having diſpatched the dog, by ſhooting him through the hinder part of his head, I examined one of his eyes (both of which had an uniform blueiſh caſt, like a film over the pupil) and found all the three humours perfectly tranſparent, and, as far as could be judged, in their right ſtate; but the *cornea* was throughout white and opaque, like a bit of griſtle, and remarkably thick.

BEFORE this experiment, I had imagined, that animals ſtruck blind by lightning had probably a *gutta ſerena*, on account of the concuſſion which is ſeemingly given to the nervous ſyſtem by the electric ſhock; whereas this caſe was evidently an inflammation, occaſioned by the exploſion being made ſo near the eyes, terminating in a ſpecies of the *albugo*; but which I ſuppoſe would have been incurable. One of the eyes of this dog was affected a little more than the other; owing, probably, to the ſtroke being made a little nearer to one eye than the other. I intended to give the ſtroke about an inch above the eyes.

IN order to aſcertain the effects of electricity on an animal body, I, after this, began a courſe of experiments on the conducting power of its conſtituent parts; and for ſome time imagined that a piece of ſpinal marrow of an ox conducted ſenſibly worſe than the muſcular fleſh; but after a great number of trials with pieces of ſpinal marrow from various animals, and pieces of muſcular fleſh, of the ſame ſize and form, and in various ſtates of moiſture and dryneſs, I gave up that opinion as fallacious; but I cannot help wiſhing the experiments were reſumed with ſome more accurate meaſure of conducting power than hath yet been contrived.

BEING willing to obſerve, if poſſible, the immediate effect of the electric ſhock on the heart and lungs of animals, I gave, June the 5th, a ſhock from ſix ſquare feet to a frog, in which the thorax had been previouſly laid open, ſo that the pulſation of the heart might be ſeen. Upon receiving the ſtroke, the lungs were inſtantly inflated; and, together with the other contents of the thorax, thrown quite out of the body. The heart, however, continued to beat, though very languidly, and there was no other ſign of life for about ten minutes. After that, a motion was firſt perceived under its jaws; which was propagated, by degrees, to the muſcles of the ſides; and at laſt the creature ſeemed as if it would have come to life, if it had not been ſo much mangled. The ſtroke entered the head, and went out at the hind feet.

June the 6th. I diſcharged a battery of thirty-three ſquare feet through the head and whole extended body of another frog. Immediately upon receiving the ſtroke, there was, as it were, a momentary diſtention of all the muſcles of the body, and it remained ſhrivelled up in a moſt ſurpriſing manner. For about five minutes there appeared no ſign of life, and the pulſation of the heart could not be felt with the finger. But afterwards, there firſt appeared a motion under the jaws, then all along the ſides, attended with convulſive motions of the other parts, and in about an hour it became, to all appearance, as well as ever.

The ſame day, I gave the ſame ſtroke to two other frogs. They were affected in the ſame manner, and perfectly recovered in leſs than three hours.

These facts ſurpriſed me very much. I attribute the recovery of the frogs partly to the moiſture, which always ſeems to cover their body, and which might tranſmit a good part of the ſhock; and partly to that proviſion in their conſtitution, whereby they can ſubſiſt a long time without breathing. To aſcertain this, I would have given the ſhock to toads, ſerpents, fiſhes, &c. and various other exanguious animals, but I had not an opportunity. Beſides, it is paying dear for philoſophical diſcoveries, to purchaſe them at the expence of humanity.

SECTION IX.

EXPERIMENTS ON THE CIRCULAR SPOTS MADE ON PIECES OF METAL BY LARGE ELECTRICAL EXPLOSIONS.

IN the courſes of experiments with which I ſhall preſent the reader in this and the two following ſections, I can pretend to no ſort of merit. I was unavoidably led to them in the uſe of a very great force of electricity. The firſt new appearance was, in all the caſes, perfectly accidental, and engaged me to purſue the train; and the reſults are ſo far from favouring any particular theory or hypotheſis of my own, that I cannot perfectly reconcile many of the various phenomena to any hypotheſis.

FROM the firſt of my giving any particular attention to electrical experiments, I entertained a confuſed notion, that a perſon would ſtand the beſt chance for hitting upon ſome new diſcovery, by applying a greater force than had hitherto been uſed. Conſidering the prodigious number of electrical machines that were in the hands of ſo many ingenious men, in different parts of the world, I imagined that all that could be done *in little* had been tried; and that the uſual experiments had been diverſified and combined in almoſt every method poſſible; whereas, ſince electrical machines, I obſerved, had, of late years, been gradually reduced into leſs and leſs compaſs, a great power of electricity would be almoſt a new thing, and might therefore ſupply the means of new experiments. Even Dr. Franklin's force, I conſidered, was ſmall, in compariſon of what might eaſily be raiſed, and without a very great expence.

With theſe general and random expectations, I kept gradually increaſing my quantity of coated glaſs, till I got a battery of thirty, forty, ſixty, and at length near eighty ſquare feet; and the reader will, in ſome meaſure, have ſeen already, that I was not wholly diſappointed in it. The following courſes of experiments are more remarkable inſtances of the advantage I derived from this power of electricity.

The firſt remarkable fact that I was by this means led to diſcover, is that of the circles with which pieces of metal that receive electrical exploſions are marked. I ſhall faithfully relate all the circumſtances, and varieties in which it has been exhibited, and the obſervations I have made upon it; and this I cannot do better than by writing the narrative, in the order in which the appearances occurred.

June the 13th, 1766. After diſcharging a battery, of about forty ſquare feet, with a ſmooth braſs knob, I accidentally obſerved upon it *a pretty large circular ſpot*, the center of which ſeemed to be ſuperficially melted, in a great number of dots, larger near the center, and ſmaller at a diſtance from it. Beyond this central ſpot was a circle of black duſt, which was eaſily wiped off; but, what I was moſt ſtruck with was, that, after an interruption of melted places, there was an intire and exact circle of ſhining dots, conſiſting of places ſuperficially melted, like thoſe at the center. The appearance of the whole, excluſive of the black duſt, is repreſented, Plate I. fig. 5. No. 1.

June the 14th. I took the ſpot upon ſmooth pieces of lead and ſilver. It was, in both caſes, like that on the braſs knob, only the central ſpot on the ſilver conſiſted of dots diſpoſed with the utmoſt exactneſs, like *radii* from the center of a circle, each of which terminated a little ſhort of the external circle.

EXAMINING the ſpots with a microſcope, both the ſhining dots that formed the central ſpot, and thoſe which formed the external circle, appeared evidently to conſiſt of *cavities*, reſembling thoſe of the moon, as they appear through a teleſcope, the edges projecting ſhadows into them, when they were held in the ſun.

THE moſt beautiful appearance of this kind was exhibited by a ſpot, which I took on a gold watch caſe. Beſides the cavities, there were, in ſeveral places of the ſpot, hollow *bubbles* of the metal, which muſt have been raiſed when it was in a ſtate of fuſion. Theſe looked very beautiful when examined with a microſcope in the ſun, and were eaſily diſtinguiſhed from the cavities, by having their radiant points (which were very remarkable, and dazzling to the eye) on oppoſite ſides to thoſe of the cavities, with reſpect to the ſun. The whole progreſs ſeems to have been firſt a fuſion, then an attraction of the liquid metal, which helped to form the bubbles; and laſtly the burſting of the bubbles, which left the cavities. N. B. By this exploſion half an inch of a ſteel wire, one ſeventieth of an inch in diameter, was melted, and entirely diſperſed. In the diſperſion, ſparks of it were ſeen red-hot, above half a yard from the place where the wire had lain. This circumſtance I have frequently obſerved ſince.

I TOOK the circular ſpot upon poliſhed pieces of ſeveral metals, with the charge of the ſame battery, and obſerved that the cavities in them were ſome of them deeper than others, as I thought, in the following order, beginning with the deepeſt, *tin*, *lead*, *braſs*, *gold*, *ſteel*, *iron*, *copper*, *ſilver*.

I WILL not be very poſitive as to the order of ſome of the metals, but ſilver was evidently not affected a fourth part ſo much as gold, and much leſs than any of the others. The circles were marked as plain, but the impreſſion was more ſuperficial. Qu. Is this owing to the heat being ſooner diffuſed equably through a piece of ſilver, than through the ſubſtance of any other metal?

I thought there might poſſibly be ſome difference in the circles on metals which had been a long time in a ſolid form, and thoſe which had been lately fluid; and to aſcertain it, I made the exploſion between a piece of lead juſt ſolid after melting, and another ſmooth piece, that I had kept a conſiderable time.

The piece of freſh lead was melted more than the other, but there was no other difference between them.

The ſemi-metals, as *biſmuth* and *zink*, received the ſame impreſſion as the proper metals; being melted about as much as iron.

I made three diſcharges between a piece of highly poliſhed ſteel, and a piece of very ſmooth iron; and in all the caſes thought the ſteel was more deeply melted than the iron. I mention this experiment more particularly, on account of the ſingular, and beautiful appearance of the circular ſpot upon the ſteel in two of the diſcharges. A circular ſpot, of about an eighth of an inch in diameter, was uniformly melted, and pretty well defined; and there was a ſpace round this central ſpot, of the ſame breadth, uniformly filled with ſmall melted places; but in one of them twice as large as in the other. They exhibited the exact appearance of a planet ſurrounded by a denſe atmoſphere; ſuch as, I think, I remember ſeeing the figures of, in the plates of Burnet's Theory of the Earth. The other circle upon the ſteel was a common one.

When the kitten above mentioned was killed, there was no circular ſpot, or any fuſion on the braſs knob. I have always found it the moſt perfect, when the circuit has been compoſed of the beſt conductors, and had the feweſt intervals.

JUNE the 19th. Hanging the caſe of one watch upon the braſs knobs communicating with the inſide of the battery, and receiving the exploſion from it upon the caſe of another watch, which was ſometimes of the ſame, and ſometimes of a different metal, and meaſuring the circles afterwards; I found them to be very nearly of the ſame diameter. The ſmall varieties ſeemed to be accidental; or at leaſt did not depend, either upon the metal, or the direction of the electric fluid. But I thought it pretty evident, from a great number of experiments, that the metal which communicated with the outſide of the battery, and which I held in my hand to take the exploſion, was marked the more diſtinctly of the two.

IT ſeemed that, when the battery was charged very high, the central ſpot was the moſt irregular, many of the dots which compoſed it ſpreading into the outer circle, and ſome dots appearing beyond the outer circle, and very much effacing it; ſo that the beſt way to procure a diſtinct circle, is to take a moderate charge of a very large battery. This may be the reaſon why the outer circle cannot be perceived, when only ſmall jars are uſed; the circumference of the circle being very ſmall, and the charge generally too high. In a very weak charge, it is too faint to be perceived. I have ſometimes, however, ſeen a very diſtinct circle made by only two jars, each containing half a ſquare foot of coated glaſs.

THE *diameter* of the ſpot ſeems to depend upon the quantity of coated glaſs; but in what proportion, I have not yet accurately aſcertained.

I HAVE obſerved a good deal of variety in the external circles. Sometimes they have conſiſted of pretty large dots, diſpoſed at nearly equal diſtances, in an exact circle; which, in the ſpaces betwixt each large dot, was completed by ſmaller dots, viſible only by a microſcope. But, generally, the external circle conſiſts of a ſpace full of dots, placed irregularly, but ſo that a line drawn through the midſt of them makes a pretty exact circle round the central ſpot.

Presently after I had obſerved the *ſingle circle*, I imagined that, whatever was the cauſe of the appearance, it was not improbable, but that *two or more concentric circles* might be procured, if a greater quantity of coated glaſs was uſed, or perhaps if the exploſion was received upon metals that were more eaſily fuſed than braſs. Accordingly, June the 27th, taking the moderate charge of a battery, conſiſting of about thirty-eight ſquare feet, upon a piece of tin, I firſt obſerved a ſecond outer circle, at the ſame diſtance from the firſt, as the firſt was from the central ſpot. It conſiſted of very fine points hardly viſible, except when held in an advantageous light; but the appearance of the whole was very beautiful, ſuch as is repreſented, Plate I. fig. 5. No. 2.

June the 28th. I got another double circle, on a flat pewter ſtandiſh, much plainer than the former, the outer being about the ſame diſtance from the inner, as the inner was from the outſide of the central ſpot.

Having hitherto found the circles the moſt diſtinct on metals that melt with the leaſt degree of heat, I ſoon after procured a piece of that compoſition which melts in boiling water; and having charged ſixty ſquare feet of coated glaſs, I received the exploſion with it, and found, what I was endeavouring to get, *three concentric circles*; the outermoſt of which was not quite ſo far from the next to it, as that was from the innermoſt. Al the ſpace within the firſt circle was melted; but the ſpace was very well defined, and by no means like a central ſpot, which in this caſe was quite obliterated. The appearance of theſe three concentric circles is repreſented, Plate I. fig. 5. No. 3.

I HAVE ſeveral times ſince found parts of three concentric circles upon braſs knobs, when I have uſed no more than thirty ſquare feet of coated glaſs. They ſeem to be more eaſily perceived, when the knobs are a little tarniſhed: for then the ſmall dots, in which the metal is melted, are more eaſily diſtinguiſhed, eſpecially when they are held in a proper light with reſpect to the ſun.

I MADE many attempts to make theſe circles larger than I had uſually found them upon pieces of metal, chiefly by means of worſe conductors; thinking that the electric matter not being ſo well conducted, and paſſing with leſs rapidity, would ſpread wider. This was probably the caſe, but then it is likewiſe probable, that I wanted force to make ſuch an impreſſion viſible. For this purpoſe, however, I received the exploſion between two pieces of raw fleſh, two potatoes, two moiſt bladders, and things of a ſimilar nature, but without any effect whatever; no mark at all, or, at leaſt, nothing regular remaining upon them. When I took the exploſion upon a piece of wood charcoal, it ſeemed to be melted, and run in ſmall heaps, within the ſpace of about the uſual diameter of a circular ſpot; and when I took it upon a piece of pit charcoal, a piece ſeemed to be ſtruck out of it, and a hole was left in it; but there was no regular circle upon either of them; nor was there any ſenſible ignition in either caſe.

AT one time I laid a piece of *lead ore*, ſcraped very ſmooth, upon the wires of the battery, and took the exploſion with a piece of tin ore ſcraped in the ſame manner; but though I examined the places with a microſcope, I could not be ſure that there was any part melted, much leſs any regular circular ſpot; but there lay on both of them a yellow matter, like ſulphur, round the place where the exploſion was taken, and a very diſagreeable ſmell was excited. This probably aroſe from a mixture of the ſulphur of the lead ore, and the arſenic of the tin ore.

I received the exploſion *in vacuo*, at the diſtance of about three inches; but found no regular circular ſpot, owing, probably, to the two interruptions I was obliged, in this caſe, to make in the circuit, one in the air, and the other within the receiver; by means of which the effect of both would be weakened, the whole force being, as it were, divided between them: for in all ſuch caſes, though both the exploſions were made in the open air, I found the circles leſs perfect.

Afterwards, I contrived to make the exploſion in one additional atmoſphere of condenſed air, but the circles were ſmaller, and leſs diſtinct than the other two circles, which I was obliged, at the ſame time, to make at the other interruption of the circuit, in the open air. The denſer air would probably confine the electric matter within a narrower compaſs; in the ſame manner as the common air prevents that diffuſion of it which is remarkable *in vacuo*.

The diſtance at which the diſcharge was made occaſioned no difference in the diameter of theſe circular ſpots. When, by putting a drop of water upon the braſs rod communicating with the inſide of the battery, I made the diſcharge at the diſtance of two inches, the ſpot was juſt the ſame as if it had been received at the diſtance of half an inch, i. e. about a quarter of an inch in diameter.

I always found that if the exploſion was obliged to paſs through any bad conductor before it reached the metal, the impreſſion it made upon it was contracted, and deeper than if it had been received immediately by the metal. This was evident when paper, a piece of bladder, or varniſh were put upon the braſs rods with which the diſcharges were made; though a very thin coating of varniſh or moiſture did not intirely prevent the appearance of the circles.

In making a courſe of experiments with bad conductors, and in uſing various methods to promote the diſcharge of the battery at greater diſtances than uſual, I was peculiarly ſtruck with ſome phenomena which occurred in the uſe of water.

I put a drop of water, about a quarter of an inch in diameter, upon the braſs rod communicating with the inſide of the battery, and took the exploſion directly over it. The diſcharge was made at the diſtance of about an inch, and the extremity of the drop was marked with a moſt beautiful circle, exceedingly well defined on the inſide, and vaniſhing gradually outwards, like a fine ſhade in drawing. But what ſtruck me moſt in the appearance was, that, in this circle, there was no central ſpot.

Not knowing what this new circumſtance was owing to, I wetted a piece of ſmooth copper, which lay upon the wires of the battery, and taking the exploſion upon it, I only found a long ſtreak at the edge of the wetted place, well defined on the ſide of the water, but vaniſhing gradually on the oppoſite ſide, as in the former caſe. In this, and other ſimilar experiments, I obſerved that the electric matter avoided the water, and would go a greater way in the air, in order to come at the metal.

I then laid more water upon the copper, but ſo as only to moiſten it; for the ſurface, being convex, would not allow it to lie in any great quantity; and upon taking the exploſion, I found no circle, but ſeveral beautiful circular ſpots melted very deep, one of which was much larger than the reſt. Theſe experiments ſeem to ſhow, that the electric matter meets with a conſiderable reſiſtance in paſſing through water, which confines its excurſion more than the air; and that, by ſuch a condenſation, its force is greatly increaſed, ſo as to leave deeper impreſſions upon the metal than when it had paſſed only through the air; in like manner, if two pieces of metal be placed, nearly in contact, or if they be light, and one of them lie upon the other, the impreſſion made upon both of them, by the diſcharge of the battery paſſing through them, will be conſiderably deeper than it would have been if the electric matter had not been confined to ſo ſmall a compaſs as the points in contact.

To account for the formation of theſe concentric circles, nothing ſeems to be neceſſary, but the ſuppoſition of the elaſticity of the electric fluid, whereby its particles repel one another. For then, ſuppoſing a quantity of electric matter to iſſue from one piece of metal to another, through the air, it will endeavour to ſpread, but will be confined in its paſſage by the ſurrounding electric medium, and the ſtrong attraction of the oppoſite metal. If this piece of metal have a flat ſurface, or one that is nearly ſo, the fluid will be attracted by it pretty equally, within a certain ſpace; ſo that the mutual repulſion of its particles will have room to exert itſelf, and produce a diviſion of the whole quantity; and as this repulſion is the ſame in all directions, the effect muſt be its throwing itſelf into a circle, or ſeveral concentric circles, on its entering the oppoſite piece of metal, and conſequently melting it, in that form. For the ſame reaſon the circles themſelves will conſiſt of ſeparate dots, each of which might have been cauſed by the fluid in another hollow circle, but being ſo ſmall, the fuſion of the metal could not ſhow that circumſtance.

Of the circles being formed in this manner, I have been in a manner an eye witneſs, when I have preſented a flat piece of metal to a large prime conductor ſtrongly electrified, and have ſeen the large ſparks, five or ſix inches long, divide about the middle, and ſtrike the metal in a circle, about an inch in diameter;

ter; generally with a central ſpot, but ſometimes without one.

The manner in which ſeveral of the jars, mentioned in a former ſection, were broken ſeems to be analogous to the formation of theſe circles. I mean thoſe that were pierced with a number of ſmall holes in the neighbourhood of the principal one; but more eſpecially that which was broken with an intire circle of ſmall and independent fractures round the principal hole.

The remarkable ſtory of the five peaſants of whom the firſt, third, and fifth were killed by lightning, as they were walking in a right line; and which was mentioned before, as analogous to a fact obſerved by Mr. Monnier, will perhaps be thought more analogous to this. For ſuppoſing the diameter of the concentric circles formed by lightning to be ſufficiently great, and the central ſpot to fall upon the third perſon, the two on each ſide of him would eſcape, by being in the firſt interval round the central ſpot; while the two who walked firſt and laſt would fall into the circumference of the firſt circle. But other facts lead me to think, that all theſe effects may have been produced by a conſtant ſtream of electric matter, in a progreſſive motion, the firſt part of which being diſcharged upon any object, the ſtream is weakened when it meets with the ſecond, but is accumulated again when the third comes in its way, and ſo alternately, as long as the ſtream and the line of objects continues. This account ſeems to be rendered more probable by the number of objects that have been ſtruck in this manner, particularly in a caſe which is related by Dr. Wallace in his account of the Orkney iſlands, p. 78. "In "the year 1680," he ſays, "the lightning entered a gentle-"man's cow-ſtall where were twelve cows ſtanding ſide by "ſide, as they uſed to be, and killed every other one; that

" is, it killed the firſt, and miſſed the ſecond; it killed the " third and miſſed the fourth; and ſo of the reſt, ſo that ſix " were killed, and ſix remained alive and untouched."

COMMUNICATING this experiment to Dr. Price, he ſuggeſted to me, that the circles called *fairy-rings*, which conſiſt of graſs of deeper green in paſture fields, and which have by ſome been imagined to be occaſioned by lightning, might be analogous to the circles above mentioned, but that they want a central ſpot. I have ſince examined one of theſe rings. It was about a yard in diameter, the ring itſelf about a quarter of a yard broad, and equally ſo in the whole circumference; but there was no appearance of any thing to correſpond to the central ſpot.

I HAVE ſince met with a curious article in the Philoſophical Tranſactions, relating to thoſe fairy circles, communicated by Mr. Jeſſop, which confirms the ſuppoſition of their being occaſioned by lightning, and with which I ſhall therefore conclude this ſection.

" I HAVE often been puzzled to give an account of thoſe pheno- " mena, which are commonly called fairy-circles. I have ſeen " many of them, and thoſe of two ſorts; one ſort bare, of ſeven " or eight yards diameter, making a round path ſomething more " than a foot broad, with green graſs in the middle; the others like " them, but of ſeveral bigneſſes, and encompaſſed with a cir- " cumference of graſs, about the ſame breadth, much freſher " and greener than that in the middle. But my worthy friend " Mr. Walker, gave me full ſatisfaction from his own experience; " it was his chance one day, to walk out among ſome mowing " graſs (in which he had been but a little while before) after a " great ſtorm of thunder and lightning; which ſeemed by the " noiſe and flaſhes to have been very near him: he preſently ob- " ſerved a round circle, of about four or five yards diameter, the " rim whereof was about a foot broad, newly burnt bare, as the

" colour and brittleneſs of the graſs roots did plainly teſtify. He
" knew not what to aſcribe it unto but to the lightning, which,
" beſides the odd capricios remarkable in that fire in particular,
" might without any wonder, like all other fires, move round,
" and burn more in the extremities than the middle. After the
" graſs was mowed, the next year it came up more freſh and
" green in the place burnt, than in the middle, and at mowing-
" time was much taller and ranker *."

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* Phil. Tranſ. abridged, Vol. ii. p. 182.

SECTION X.

EXPERIMENTS ON THE EFFECTS OF THE ELECTRICAL EXPLOSION DISCHARGED THROUGH A BRASS CHAIN, AND OTHER METALLIC SUBSTANCES.

FROM the very firſt uſe of my battery, I had obſerved a very *black ſmoke* or *duſt* to ariſe upon every diſcharge, even when no wire was melted, and the braſs chain I made uſe of was of a conſiderable thickneſs. Of this circumſtance, however, I only made a ſlight memorandum, as what I could not then account for, and paid no particular attention to it; till on the 13th of June 1766, I was ſtruck with another caſual appearance, as I was intent upon the experiments relating to the circles above mentioned.

I OBSERVED, that a piece of white paper, on which lay the chain I was uſing to make the diſcharge, was marked with a *black ſtain*, as if it had been burnt, wherever the links had touched it. Yet I could not then think that it could be burnt by ſo thick a chain. I imagined the chain muſt have been dirty, and the dirt have been ſhaken off by the ſtroke. Still however I neglected the experiment till, obſerving a very ſtriking appearance of the ſame kind, on the 1ſt of September following, I was determined to attend to the circumſtances of it a little more particularly than I had done.

I MADE my chain very clean, and wrapping it in white paper, I made a diſcharge of about forty ſquare feet through it, and found the ſtain wherever it had touched the paper.

SOME time after, I wrapped the paper, in the ſame manner, round a piece of braſs wire; but, making a diſcharge through it, ſaw no ſtain. To aſcertain whether this appearance depended upon the diſcontinuity of the metallic circuit, on the 13th of the ſame month, I ſtretched the chain with a conſiderable weight and found the paper, on which it lay as the ſhock paſſed through it, hardly marked at all.

FINDING that it depended upon the diſcontinuity, I laid the chain upon white paper, making each extremity faſt with pins ſtuck through the links; and when I had made the diſcharge, obſerved that the black ſtains were oppoſite to the *body of the wire* that formed the chain, and not to the *intervals*, as I had ſometimes ſuſpected.

SEPTEMBER the 18th. Obſerving that a pretty conſiderable quantity of black matter was left upon the paper, on every diſcharge with the ſame chain; I imagined it muſt have loſt weight by the operation; and to aſcertain this circumſtance, I took another chain not ſo thick as that I had uſed before. It was five feet four inches long, and weighed exactly one ounce, ſeventeen penny-weights, four grains. After the diſcharge, I found it had loſt exactly half a grain of its weight. The ſhock had only paſſed through a part of it, the reſt lying on a heap. I then diſcharged the ſame ſhock through its whole length, and weighed it, found it had loſt juſt another half grain. By repeated experiments I found, afterwards, that the ſureſt way to ſtrike off part of its weight, was to make the ſhock paſs through a ſmall part of its length, and that when a conſiderable length was uſed the event was uncertain.

N. B. THESE and all the following experiments, except where the contrary is expreſſed, were made with a battery of *thirty-two ſquare feet*, that force appearing to be ſufficient, and the charging of it not taking up much time. At the time of both the above mentioned diſcharges, an iron wire of one ſeventieth of an inch in diameter was made red-hot, but was not melted.

OBSERVING how deep a ſtain was made by the links of a thick braſs chain, I had the curioſity to try what would be the conſequence of ſending a ſhock through a piece of charcoal. Accordingly I took a ſmall piece, about half an inch in length, and found that, in the diſcharge, it was all blown to duſt. The paſteboard on which it lay was torn, the charcoal being forced into it, ſo that the impreſſion appeared on the other ſide. The blackneſs was ſpread to a great diſtance, and the tinge every where indelible.

SEPTEMBER the 21ſt. In making the mark above mentioned, on part of the ſheet of paper, on which I had written an account of the experiment to Dr. Franklin, I happened to lay the chain ſo as to make it return at a ſharp angle, in order to impreſs the form of a letter on the paper; and obſerved that, upon the diſcharge, the part of the chain that had been doubled was diſplaced, and pulled about two inches towards the reſt of the chain. At this I was ſurpriſed, as I thought it lay ſo, as that it could not ſlide by its own weight. Upon this I repeated the experiment with more accuracy. I ſtretched the whole chain along a table, laying it double all the way, and making it return by a very ſharp angle. The conſequence always was, that the chain was ſhortened about two inches, and ſometimes more; as if a ſudden pull had been given to it by both the ends.

Considering that this pull muſt have been given to it by the ſeveral links ſuddenly repelling one another, at the inſtant of the exploſion, I compared the links with the black marks that were made by them upon the table, and found that each link had been pulled from the place on which it had lain, and moſt of all, at the greateſt diſtance from the place of the exploſion.

Convinced that the chain had been ſhortened by the mutual repulſion of the links, I endeavoured to meaſure with exactneſs how much the ſhortening was, in a given length of chain. To do this, I meaſured two feet four inches of the chain, as it lay upon the table, in one ſtraight line, without any return, one end being fixed and the other moveable; and found that, upon diſcharging ſixty-four ſquare feet through it, it was ſhortened a quarter of an inch in its whole length. I had contrived that the ſuddenneſs of the motion ſhould not throw one part of the chain upon the other.

Suspecting that the black ſmoke, which roſe at every diſcharge, might come, not from the chain, but from the paper, or the table on which it lay, and which was probably burnt by the contact of it, I let the chain hang freely in the air; but, upon making the diſcharge, I obſerved the ſame black groſs ſmoke that had before riſen from the paper or the table. It was therefore part of the metal itſelf, which had been converted into that black duſt.

To give my reader a better idea of the mark made upon white paper by a chain, through which the electric ſhock is tranſmitted, I laid a chain upon the original drawing of Plate I. for the engraver to copy as exactly as he could; and he has ſucceeded pretty well. The breadth of the ſpots are about the mean thickneſs of the wire of the chain, and [*a*, *b*] marks the place to which that part of the chain which was returned was thrown back, by the ſudden repulſion of the links.

I had before obſerved the electric ſparks betwixt each link to be moſt intenſely bright, ſo as, ſometimes, to make the whole chain appear like one flame in the dark; but the appearance of the chain at the inſtant of the ſhock, as it hung freely in the air, was exceeedingly beautiful; the ſparks being the largeſt and brighteſt at the bottom, and ſmaller, by degrees, towards the top, where they were ſcarcely viſible; the weight of the lower links having brought them ſo much nearer together.

September the 26th. Being ſtill in ſome doubt whether the blackneſs that was left on the paper came from the burning of the paper, or ſomething that was thrown from the chain; I once more hung the chain freely in air, and put under it, but ſo as not to touch it, a piece of white paper, on which I alſo laid a few pieces of down, to obſerve whether they would be affected by any electrical attraction or repulſion. On making the diſcharge, the down was all diſperſed, and the paper was marked with a black ſtain, near the length of an inch; which was the diſtance at which the two parts of the chain hung from one another, a little above the paper. Some parts of the ſtain were deeper than others, the whole mark conſiſting of four different ſpots of a deeper black, joined by fainter ſtreaks, anſwering to four links of the chain, which hung nearly parallel to the paper. The ſtain could not be wiped off with a handkerchief, though it was not ſo deep as when the chain had touched the paper. Thus I was ſatisfied, that a conſiderable part, at leaſt, of the blackneſs had come from the chain.

September the 27th. Willing to aſcertain more exactly what part of the chain, the ſolid links, or the intervals, was moſt affected by the ſhock; I dipped it in water, and laying it quite wet upon a piece of white paper, diſcharged a ſhock through it. Part of the water was thrown into my face, being ſcattered in all directions, and all the chain left inſtantly and perfectly dry. The paper was very much ſtained for the ſpace of an inch broad, wherever the chain lay; not equably, but as if it had been handled with dirty fingers. The ſtain was indelible, and where the chain was returned, a hole was ſtruck quite through the paper.

To determine whether the paper, in the above mentioned inſtances, had really been *burnt*, as well as *ſtained*. I laid a part of the chain, at the time of the laſt diſcharge, upon three half crowns; and found they were all melted, in the places where the chain had touched them. The marks made by the fuſion were about the breadth of the chain, and ſo deep that nothing but a tool could efface them.

To determine, if poſſible, more ſenſibly what it was that made the black tinge, I laid the chain upon my hand, when I had a moderate charge; and it was marked juſt like the paper. I felt a kind of pricking or burning at the inſtant of the exploſion, and the painful ſenſation continued a ſmall ſpace of time.

I MADE no doubt but that with a heat that melted metals, I could eaſily contrive to fire gunpowder; but, though I laid the chain upon the grains, and rammed the powder about the chain put through a quill, I could not ſucceed. In the firſt caſe, the powder was diſperſed; and in the ſecond, the quill was burſt, and there was a ſmell, as after an exploſion of gunpowder, but no actual firing of it.

HITHERTO I had always put the chain in contact with bodies that were conductors. I was now willing to try what would be the conſequence of laying it in contact with electrics. Accordingly, I dipped the chain in melted roſin, till it had got a coating of a conſiderable thickneſs. When it was quite ſtiff, I laidit carefully, without bending, upon white paper, and made the diſcharge

charge through it. The roſin was inſtantly diſperſed from all the outſide of the chain, it being left as clean as if none had ever been put on. That with which the holes in the chain had been filled, having been impelled in almoſt all directions, was beaten to powder; which, however, hung together, but was perfectly opaque; whereas it had been quite tranſparent, before this ſtroke. I felt ſome of the roſin fly in my face. The ſtain upon the paper was very deep, containing a good deal of roſin, and ſeveral holes were ſtruck through the paper on which it was laid. A half crown, on which part of the chain had lain, was melted, and ſo deeply ſtained with the roſin, that it could not eaſily be cleaned.

I NEXT laid the chain upon a *piece of glaſs*; and conſidering how both the half crown and the roſin had been affected, expected it would have been broken to pieces; but inſtead of that, the glaſs was marked in the moſt beautiful manner, wherever the chain had touched it; every ſpot the width and colour of the link. The metal might be ſcraped off the glaſs at the outſide of the marks; but in the middle part it was forced within the pores of the glaſs; at leaſt nothing I could do would force it off. On the outſide of this metallic tinge was the black duſt, which was eaſily wiped off.

I HAVE ſince given the ſame tinge to glaſs with a ſilver chain, and ſmall pieces of other metals; but could not do it with large pieces. They were melted where they touched one another, but the glaſs was not tinged.

OCTOBER the 7th. I had the curioſity to try, whether I could not give a tinge to glaſs with quickſilver. In order to this, I laid ſome globules in a right line, and laid a thin piece of glaſs upon them, to flatten them, and bring them nearer into contact with one another. Both the ſlips of glaſs were ſhattered in a thouſand pieces, and diſperſed all over the room, ſeveral of them flying in my face; though no part of the quickſilver could be found, except what adhered to ſome fragments of the glaſs, to which it had given a kind of uniform whiteneſs; but no diſtinct globules could be ſeen, and it was eaſily wiped off, ſo that no part of it was fixed in the glaſs. My head ached all the remainder of the day, which I attributed to the fumes of the mercury.

SEPTEMBER the 28th. Having dipped the chain in water, and found it inſtantly diſperſed, I wiſhed to ſee what would be the effect of diſcharging a ſhock through a chain quite covered with water. Accordingly, little imagining the conſequence, I laid the chain upon a piece of white paper, in the bottom of a china diſh, and poured in water juſt ſufficient to cover it. Alſo, under one part of the chain, and in the water, I put a half crown. Upon the exploſion, the water was blown about the room, to a great diſtance, the half crown was melted in two places, the diſh broken into many pieces, and the part that lay immediately under the chain into very ſmall fragments. The paper was a little ſtained, and the water, I could perceive, had been a little fouled by the black duſt.

BEING certain that the diſh muſt have been broken by the concuſſion given to the water by the electric ſpark under it, in the manner in which Signior Beccaria's tubes were broken (though I had not ſeen his work at that time, but had ſeen the experiment at Mr. Lane's *) I had the curioſity to try what would be the effect of making a diſcharge through the chain hanging freely in water. I therefore got a tin veſſel, holding a quart, and letting the chain hang three inches and a half below the ſurface of the water, made the diſcharge. The electric ſparks appeared intenſely bright in the water, all along the chain; ſome of the water was thrown

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* See his account of it, Phil. Tranſ. Vol. lvii. p. 458.

out, and the veſſel appeared to have been preſſed with ſome force upon a book, which I had put under it, a viſible impreſſion being made upon it. The veſſel muſt have received a great concuſſion: for the duſt had been ſhaken from the bottom upon the book, though I had carried the veſſel up and down the room, without perceiving that any duſt adhered to it.

I was willing to repeat this experiment with ſome variation of circumſtances, and faſtened a piece of ſmall ſilver wire to two pieces of ſtrong braſs wire, and plunged the whole an inch or two under the ſurface of the water. Upon the diſcharge, the ſilver wire was melted, at leaſt ſnapped aſunder, the veſſel had been preſſed downwards more violently than before, a conſiderable quantity of the water was thrown about the table, and ſome was daſhed perpendicularly upwards, againſt the top of the room; where there were five wet places, each about the bigneſs of a half crown. I have ſince frequently melted wires under water, and have even made large pieces of iron wire red-hot in the water.

September the 29th. I made a diſcharge through three pieces of the ſame chain, each being a different circuit. They all left their impreſſion upon the paper, and nearly equal. Alſo three out of four pieces made pretty equal marks, but the fourth failed intirely.

At another time, a chain, which communicated with the outſide of the battery, but which made no part of the circuit, made the black ſtain on a piece of white paper on which it accidentally lay, almoſt as deep as the chain that formed the circuit. I was then melting a piece of wire, which had the ſame effect as uſing a bad conductor. The ſame thing has frequently happened ſince.

November the 12th. I put a chain through a glaſs tube, ſo wide as that it could only touch one ſide; and upon the diſcharge,

charge, obſerved four ſets of marks, made by the metal being driven into the glaſs; as if four chains had been in the tube, and all had received the ſhock. Two of the rows, on one of which I imagine the chain had lain, were better marked than the other two, but all were very plain.

THE laſt thing that engaged my attention with reſpect to this courſe of experiments, was that *black duſt* which I have obſerved to be diſcharged from the braſs chain, and other pieces of metal. As it was ſo extremely light as to riſe like a cloud in the air, ſo as ſometimes to be viſible near the top of the room; I concluded that it could not be the metal itſelf, but probably the *calx*, or the calx and *phlogiſton*, in another kind of union than that which conſtitutes a metal; and that the electric exploſion reduced metals to their conſtituent principles as effectually as any operation by fire could do it, and in much leſs time. I was confirmed in this opinion by finding, in the firſt place, that this black duſt collected from a braſs chain would not conduct electricity, which is known to be a property of the calces of metals, and alſo by the reſult of ſome of the following experiments.

CONSIDERING this black duſt as a proof of calcination, and obſerving it to be produced when I made the exploſions for the circular ſpots between gold and ſilver watch-caſes, as was related above; I began to think I had made a calcination of thoſe metals, which all the chymiſts ſay is impoſſible: but the following experiments convinced me, that it could only be the alloy that was in them which had yielded the black duſt or calx.

SENSIBLE that my experiments with theſe metals would conclude nothing, unleſs I got the ſpecimens quite pure, I firſt procured a ſmall quantity of *grain gold*, which I was informed was the pureſt that the goldſmiths know, and diſcharged an explosion of the battery through a train of the pieces, an inch and a half in length, laid on a piece of white paper. Only two of the larger grains could be found after the explosion. Two leaves of paper were burnt, or torn through in ſeveral places, and more would probably have been torn in the ſame manner, if I had uſed more. But what I principally attended to was the tinge that was given to the paper, with a view to which I had made the experiment. The paper was ſtained near an inch on each ſide of the train, with black intermixed with red, making an odd motley appearance.

With the ſame view, I laid a ſimilar train of bits cut with a knife from a piece of as pure ſilver as I could procure. They were diſperſed, and the paper burnt through, in the ſame manner as with the gold; and the ſpace of about an inch on each ſide of the train was ſtained with black intermixed with a deep yellow, which was conſiderably different from the tinge made by the fuſion of the gold.

The blackneſs in theſe tinges convinced me, that there had been a calcination of ſome part of the metal; but I was convinced it muſt have been ſome alloy, by an experiment I preſently after made with a piece of leaf gold; which, I believe, is generally the pureſt that can be got. A ſmall ſlip of this I put through a quill, letting a part hang out at each end; and when I had made the diſcharge through it, I found the quill tinged with a beautiful *vermillion* red, without the leaſt intermixture of black. When I diſperſed a ſlip of leaf braſs in the ſame manner, the greater part of the tinge was black, with a little brown mixed with it in a few places.

In order to aſcertain whether the black duſt was a pure calx, or contained a portion of the metal, I procured a ſmall quantity of it, by ſending an exploſion through ſome pieces of iron wire, ſometimes put into a quill, ſometimes laid upon white paper, and ſometimes upon glaſs, or incloſed in glaſs tubes; but could never be quite ſure that there was any part of it that was not affected with the magnet, which the mere calx would not have been.

SOME of the experiments with the braſs chain, related in this ſection, are ſimilar to one of Mr. Wilſon's, mentioned p. 96, concerning bodies placed without the electric circuit being affected with the exploſion. As to the cauſe of this, and the other appearances above mentioned, I have no conjecture worth communicating to the public. I have only purſued the analogy of facts, and that not very far. Others may compare them, purſue them farther, and aſcertain their cauſes.

MR. CANTON has ſince clearly proved the calcination of pure gold and ſilver by the heat of electrical exploſions, producing numberleſs moſt beautiful globules of tranſparent glaſs, and alſo others tinged with all the varieties of colour from thoſe metals. He has alſo made it probable, that the *black duſt* mentioned in this ſection, is the calx, or glaſs of the metal, reduced to ſmaller particles than the laws of optics require to produce colour.

SECTION XI.

EXPERIMENTS ON THE PASSAGE OF THE ELECTRICAL EXPLOSION OVER THE SURFACE OF SOME CONDUCTING SUBSTANCES, WITHOUT ENTERING THEM.

I OBSERVED, in relating the experiments on ice, that, in my attempts to aſcertain its conducting power, I ſometimes ſaw the flaſh of the electrical exploſion ſtrike directly to the chain, along the ſurface of the ice. But as this paſſage on the ſurface was produced only by a common jar, it was not much greater than the diſtance at which the diſcharge was uſually made, and the appearance did not ſtrike me. But afterwards the ſame phenomenon occurred in the uſe of my battery, where the paſſage over the ſurface ſo far exceeded the uſual diſtance of a common diſcharge, that it engaged my attention in a very particular manner, and produced ſome pleaſing experiments; which I ſhall recite in the manner, and nearly in the order in which they happened.

DECEMBER the 11th. Thinking to make a circular ſpot on a piece of raw fleſh, I took a leg of mutton, and laying the chain that communicated with the outſide of the battery over the ſhank of it took the exploſion on the outward membrane, about ſeven inches from the chain; but was greatly ſurpriſed to obſerve the electric fire not to enter the fleſh, but to paſs, in a body, along the ſurface of it, to come to the chain.

THINKING that this effect might be occaſioned by the fatty membrane on which the exploſion was taken, I again laid the chain, in the ſame manner, over the ſhank, and took the exploſion upon the fibres of the muſcles, where they had been cut from the reſt of the body; but ſtill the fire avoided entering the fleſh, made a circuit of near an inch round the edge of the joint, and paſſed along the ſurface, to come to the chain as before; though the diſtance was near eleven inches.

IMAGINING this effect was promoted by the chain lying lightly on the ſurface of the fleſh, and therefore not really in contact with it; I took another exploſion, when the hook of the chain was thruſt into the fleſh; on which the fire entered the mutton, and, as I held it in my hands, both my arms were violently ſhocked up to my ſhoulders; whereas, in the caſes of the electric fire paſſing over the ſurface of the fleſh, my fingers, happening to touch the chain, were only affected with a ſlight pricking, or ſuperficial burning, which has been explained before.

THIS phenomenon being ſo remarkable, and the battery by this means diſcharging at a diſtance about twenty times greater than it could uſually be made to do, I thought to try other ſubſtances, of a conducting power ſimilar to that of raw fleſh; and of theſe, *water* was the moſt obvious. Accordingly, the next day, I laid a braſs rod communicating with the outſide of the battery very near the ſurface of a quantity of water (to reſemble the chain lying upon the ſurface of the fleſh, without being in contact with it) and, by means of another rod furniſhed with knobs, made a diſcharge on the ſurface of the water, at th diſtance of ſeveral inches from any part of the rod; when the electric fire ſtruck down to the water, and, without entering it, paſſed viſibly over its ſurface, till it arrived at that part of the rod which was neareſt to the water, and the exploſion was exceedingly loud. If the diſtance at which I made the diſcharge exceeded ſeven or eight inches, the electric fire entered the water, making a beautiful ſtar upon its ſurface, and yielding a very dull ſound.

The reſemblance between this paſſage of the electric matter over the ſurface of the water, and that which Dr. Stukeley ſuppoſed to ſweep the ſurface of the earth, when conſiderable quantity of it is diſcharged to the clouds during an earthquake, immediately ſuggeſted to me, that the water over which it paſſed, and which was viſibly thrown into a tremulous motion, muſt receive a concuſſion, reſembling that which is given to the waters of the ſea on ſuch an occaſion.

To try this, myſelf, and other perſons who were preſent, put our hands into the water, at the time that the electrical flaſh above mentioned paſſed over its ſurface; and we felt a ſudden concuſſion given to them, exactly like that which is ſuppoſed to affect ſhips at ſea during an earthquake. This percuſſion was felt in various parts of the water, but was ſtrongeſt near the place where the exploſion was made.

Afterwards, I made the exploſion of a jar, containing three ſquare feet of coated glaſs, at ſome diſtance below the ſurface of the water, ſo as to be viſible in the water, and we felt the ſame concuſſion that we had done before, when the fire of the battery paſſed over the ſurface, only much weaker. The flaſh of electric fire in the water does certainly diſplace ſome of it, and thereby give a ſudden concuſſion to the reſt; and the ſimilarity of the effect is a conſiderable evidence of a ſimilarity in the cauſe.

I afterwards made the fire of a jar paſs through the water, making a ſpace of about a foot part of the circuit; when, putting our hands in its paſſage, they were affected, but in a very different manner from what they were before: for this evidently affected the nerves and muſcles of the hand internally, and occaſioned a ſmall degree of the ſame kind of convulſion which is felt by the electric ſhock itſelf; whereas the other was a mere percuſſion, affecting the ſurface of the hand. Both ſenſations were, indeed, felt moſt ſenſibly at the ſurface of the water, though our hands were, in ſome meaſure, affected by both as low as we could put them.

Being willing to experience what kind of a ſenſation this paſſage over the ſurface would occaſion, I laid a chain in contact with the outſide of a jar lightly on my finger, and ſometimes kept it at a ſmall diſtance, by means of a thin piece of glaſs; and, if I made the diſcharge at the diſtance of about three inches, the electric fire was viſible on the ſurface of the finger, giving it a ſudden concuſſion, which ſeemed to make it vibrate to the very bone; and when it happened to paſs on that ſide of the finger which was oppoſite to the eye, the whole ſeemed perfectly tranſparent in the dark. If I took the diſtance much larger, the fire entered the finger, occaſioning a very different ſenſation from the former. The one was like a blow, but of a very peculiar kind, whereas the other is well known to be a convulſion.

I then ventured to put my fingers upon a piece of the ſpinal marrow of an ox, while the exploſion of the battery was paſſing over it, when I felt only a ſlight pricking, or percuſſion on each ſide of my finger; and the ſenſation continued for ſome time. This ſenſation did not extend at all beyond the place of percuſſion; but afterwards, putting two of my fingers on the ſame piece of ſpinal marrow, when the charge of the battery was conſiderably ſtronger, I received a concuſſion which affected my whole hand, but it was with a kind of a vibratory motion.

Pleased with this reſemblance of the earthquake, I endeavoured to imitate that great natural phenomenon in other reſpects; and it being froſty weather, I took a plate of ice, and placed two ſticks, about three inches high, on their ends, ſo that they would juſt ſtand with eaſe; and upon another part of the ice I placed a bottle, from the cork of which was ſuſpended a braſs ball by a fine thread. Then, making the electric flaſh paſs over the ſurface of the ice, which it did with a very loud report, the nearer pillar fell down, while the more remote ſtood; and the ball, which had hung nearly ſtill, immediately began to make vibrations about an inch in length, and nearly in a right line from the place of the flaſh.

I afterwards diverſified this apparatus, erecting more pillars, and ſuſpending more pendulums, &c. ſometimes upon bladders ſtretched on the mouth of open veſſels; and at other times, on wet boards ſwimming in a veſſel of water. This laſt method ſeemed to anſwer the beſt of any; for the board repreſenting the earth, and the water the ſea, the phenomena of them both during an earthquake may be imitated at the ſame time; pillars, &c. being erected upon the board, and the electric flaſh being made to paſs either over the board, over the water, or over them both. This makes a very fine experiment.

When I firſt made this experiment of the electric flaſh paſſing over the ſurface of water, I thought it neceſſary, that neither the piece of metal communicating with the outſide, nor that communicating with the inſide of the jars ſhould touch the water immediately before the diſcharge. But I afterwards found, that the experiment would anſwer, though either, or even both of them were dipped in the water: for in this caſe the exploſion would ſtill prefer the ſurface to the water itſelf, if the diſtance was not very great; and would even paſs at a greater diſtance along the ſurface, when there was a nearer paſſage from one rod to the other in the water.

Just before the diſcharge, both the rods were obſerved to attract the water very ſtrongly. It was thrown upon the rod communicating with the outſide when it was laid near half an inch above the ſurface. When I put a drop of water on the rod communicating with the inſide, the diſcharge was made at the diſtance of about two inches from the ſurface of the water, the fire firſt deſcending perpendicularly, and then paſſing along the ſurface; and if the rod communicating with the outſide had a drop of water upon it, it might be placed higher over the water than if it had not. At the time of the exploſion, this drop was elongated, and promoted the diſcharge very conſiderably.

My attention was next drawn to the kind of impreſſion which was made upon the water by the paſſage of the electric fluid in this manner. To aſcertain this, I firſt placed a ſhilling level with the water, to receive the exploſion before it paſſed along the ſurface; and obſerved that it was melted, but only about half as much as I imagined it would have been in the common way. There was no regular circular ſpot. And I could never perceive that the braſs rod which communicated with the outſide of the battery was at all melted by the exploſion.

Judging from the concuſſion given to the whole body of the water over which the ſhock paſſed, I thought that the trace of it might poſſibly be preſerved on the ſurface of ſoft paſte; and accordingly I made the exploſion paſs over the ſurface of ſome, and plainly obſerved, that the part under the paſſage was depreſſed; the electric matter having repelled it. The impreſſion was not deeper where the exploſion firſt fell than in any other part of the track.

To diſtinguiſh more accurately between the effect of the electric matter when it probably enters the water, and when it only paſſes over the ſurface, I ſpread a little water, exceedingly thin, upon the ſurface of a ſmooth piece of ſlate; but, though the exploſion

ploſion paſſed over the ſurface, with its uſual violence, I could not perceive that it had occaſioned the leaſt degree of evaporation; which Signior Beccaria found to be the conſequence of making the electrical exploſion through water in ſuch circumſtances.

When the exploſion paſſed over the ſurface of the plate of ice, in the experiment of the earthquake above mentioned, the ice ſeemed to be melted, both where the chain had been laid, and alſo along the track over which the exploſion had paſſed. But this melting, if it was ſuch, was not uniform; but looked as if a chain with ſmall links had been laid hot upon it; and the impreſſion was not at all deeper where the exploſion was firſt received.

When the exploſion paſſed over the ſurface of a green leaf, the leaf was rent in two directions; the longer in the track of the exploſion, and the other at right angles to it.

I several times made the exploſion on the ſurface of ſnow, when it always diſperſed a conſiderable quantity of it, making a hole near two inches deep, and almoſt as broad as long; for it could not be made to paſs at a greater diſtance than about three inches.

I was not a little ſurpriſed to find that I could not make this electrical exploſion paſs equally over the ſurface of ſubſtances which were conductors in nearly the ſame degree; and for a long time imagined, that this property was peculiar to water, or to bodies that conducted by means of the water they contained. I could never make it paſs the ſurface of any kind of charcoal; though all the degrees of conducting power may be found in different pieces of it: and I was the more confirmed in my opinion, by obſerving, that, though the exploſion paſſed perfectly well over the ſurface of a ſmooth board, that had been juſt wetted, and immediately wiped as clean as poſſible; yet two hours after, when the board was quite dry, it would not paſs at all in the ſame place. It alſo paſſed with great violence over the ſurface of a bladder which had been moiſtened about a quarter of an hour before, and then ſeemed to be quite dry; but would not paſs in the leaſt degree two or three hours after. In the former caſe, the exploſion had left a mark where it had paſſed over, darker than the reſt of the ſurface, a kind of poliſh which was on it being taken off: in the latter caſe, as the dry bladder conducted very imperfectly, the fire of the charge ſpread in a beautiful manner, covering a ſpace of about an inch in diameter.

This electrical exploſion would not paſs in the leaſt degree, over the ſurface of new glaſs, notwithſtanding its property of diffuſion above mentioned ſeemed to promiſe that it might. Neither would it paſs at all over the ſurface of alum, rock ſalt, ſal ammoniac, blue or green vitriol, or a piece of poliſhed agate; though theſe are all conductors of a middle kind, like water; and ſeveral of them had very ſmooth ſurfaces. It alſo refuſed the ſurface of dry wood, and dry leather, even the ſmootheſt cover of a book.

But I found that I had concluded too ſoon, that this paſſage of the electrical exploſion was peculiar to the ſurface of water, by finding, firſt, that it paſſed over the ſurface of a touch-ſtone, and then over a piece of the beſt kind of iron ore, exceedingly ſmooth on ſome of its ſides. This piece is about an inch thick, and about three inches in its other dimenſions. The full charge of a jar of three ſquare feet would not enter it. It was diverting to obſerve how the electrical exploſion would make a circuit, round its angles, when it was made in a place remote from the jar. It looked like a thing invulnerable.

This electrical exploſion paſſed over the ſurface of oil of vitriol with a dull ſound, and a red colour, which was the only appearance of the kind that I have yet met with. In all other caſes, if it paſſed at all, it was in a bright flame, and with a report peculiarly loud. It paſſed over the ſurface of the moſt highly rectified ſpirit of wine without firing it; but when I took too great a diſtance, the electric fire entered the ſpirit, and the whole plate was in a blaze in a moment.

I once fancied that the fluidity of water was in a great meaſure the cauſe of this phenomenon; but I found I could not make it paſs over the ſurface of quickſilver or melted lead; though neither of the rods with which the diſcharge was made touched the metals. A dark impreſſion was made on the ſurfaces of both the quickſilver and the lead, of the uſual ſize of the circular ſpot; and remained very viſible, notwithſtanding the ſtate of fuſion in which the metals were.

So far was the electrical exploſion from paſſing over the ſurface of any metal, that I obſerved, if the diſtance through the air, in order to a paſſage through the metal, was ever ſo little nearer than the diſtance along the ſurfaces, it never failed to enter the metal; ſo that its entering the ſurface of the metal, and its coming out again ſeemed to be made without the leaſt obſtruction. If as much water was laid on a ſmooth piece of braſs as could lie upon it, it would not go over the ſurface of the water, but always ſtruck through the water into the metal. But if the metal lay at any conſiderable depth under the water, it would prefer the ſurface. It even paſſed over three or four inches of the ſurface of water as it was boiling in a braſs pot over the fire, in the midſt of the ſteam and the bubbles, which ſeemed to be no hindrance to it.

Animal fluids, of all kinds that I have tried, ſeemed in a peculiar manner to favour the paſſage of the electrical exploſion over their ſurfaces, and the report of thoſe exploſions was manifeſtly louder than when water was uſed in the experiment. This I remarked more particularly when I made uſe of milk, the white and yolk of an egg, both freſh broken, and after it had ſtood a day or two, and had contracted a hard pellicle. In all the experiments with the egg, it was obſerved, that no peculiar impreſſion was made in the place where the electric matter firſt came upon the ſurface.

It was very remarkable, that the report made by all theſe exploſions, in which the electric matter paſſed over the ſurfaces, was conſiderably louder than when the diſcharge was made between two pieces of metal; and they were obſerved by perſons at ſome diſtance out of the houſe, and in a neighbouring houſe, very much to reſemble the ſmart cracking of a whip; and indeed it would not be very eaſy to diſtinguiſh them. But the ſound made by theſe exploſions, though by far the loudeſt that I ever heard of the kind, fell much ſhort of the report made by a ſingle jar, of no very great ſize, of Mr. Rackſtrow's; who ſays that it was as loud as that of a piſtol.

It was pretty evident, that the diſtance at which the fire paſſed over animal ſubſtances was greater than it could be made on the ſurface of water; particularly in the firſt experiment of the leg of mutton. It alſo paſſed about ten inches over the ſurface of a piece of ſpinal marrow taken from an ox.

I was much ſtruck with a beautiful appearance which occurred in the courſe of theſe experiments, though it was of a different nature from them. When the electrical exploſion does not paſs over the ſurface of water, but enters the fluid, it makes a regular ſtar upon it, conſiſting of ten or a dozen rays; and, what is moſt remarkable, thoſe rays which ſtretch towards the braſs rod that communicates with the outſide of the battery are always longer than the reſt; and if the exploſion be made at ſuch a diſtance, as to be very near taking the ſurface, thoſe rays will be four or five times longer than the reſt; and a line bounding the whole appearance will be a beautiful ellipſis, one of whoſe *foci* is perpendicularly under the braſs knob with which the diſcharge is made.

IT will be in vain to attempt theſe experiments without a conſiderable force. Nothing at all, to any purpoſe, can be done with a common jar; ſince the exploſion of it will hardly paſs over the ſurface of any conductor farther than it will diſcharge through the air. The charge of a jar containing three ſquare feet of coated glaſs will not make any conſiderable appearance upon the water; and, as far as I can judge, the diſtance at which the exploſion will paſs along any ſurface is in proportion to the ſtrength of the charge. For this reaſon I make no doubt but that I could have performed all the experiments above mentioned to much greater advantage, if I had applied a greater force, but that would have required more time, and a moderate force was ſufficient to aſcertain the facts.

N. B. IN theſe experiments, I put the diſcharging rod through a handle of baked wood; by which means, I could with ſafety lay one end of it upon the wires of the battery, and make the exploſion with the other, on what ſubſtances I pleaſed.

ECTION XII.

EXPERIMENTS ON THE TOURMALIN.

FATIGUED with the inceſſant charging of the electrical battery, and ſtunned with the frequent report of its exploſion, I was deſirous of ſome reſpite from thoſe labours, and with pleaſure took up the gentle and ſilent TOURMALIN. And I make no doubt but that my readers, who muſt have ſympathized with me, will be equally pleaſed with the change.

IT was in the month of Auguſt 1766, that, being in London, I received from Dr. Heberden, who is glad to encourage every attempt in philoſophical inquiries, his ſet of tourmalins; among which was that fine one which had paſſed through the hands of Mr. Wilſon and Mr. Canton, and of which a deſcription is given in the fifty-firſt volume of the Philoſophical Tranſactions, p. 316. But notwithſtanding I had this valuable ſtone ſo long in my poſſeſſion, it was not till the latter end of December that I began to make any experiments with it, having, in the mean time, been engaged in other electrical purſuits. At length, however, having brought my other experiments to the ſtate in which the reader hath ſeen them, I was deſirous of being an eye witneſs of the wonderful properties of this ſtone, and of purſuing a few hints which had occurred to me with reſpect to it. The reſult of my experiments I ſhall lay before the reader, after having informed him in what manner, and with what precautions they were made.

The methods I uſed to apply heat to the tourmalin were various, but they will be ſufficiently explained in the particular experiments. To aſcertain the kind of electricity, I always had near me a *ſtand of baked wood*, from the top of which projected various arms for different purpoſes. Three of them were of glaſs, to two of which were faſtened threads of ſilk, as it comes from the worm, ſupporting light pieces of down; from the other hung a fine thread, about nine or ten inches long; while a braſs arm ſupported a pair of Mr. Canton's pith balls. At the other extremity of this arm, which was pointed, I could place a charged jar, to keep the balls conſtantly and equably diverging, with poſitive or negative electricity. Sometimes I ſuſpended the balls, not inſulated, within the influence of large charged jars. And laſtly, I had always at hand *a fine thread of trial* not inſulated, and hanging freely, to obſerve whether the ſtone was electrical or not when I began any experiments, and ſometimes to meaſure the ſtrength of the power which it had acquired.

Before I began any experiments, I never failed to try how long my electrometers would retain electricity, and in what degree. If the thread would retain the virtue for a few minutes, I generally preferred it, when I wanted to communicate the electricity of the tourmalin, becauſe it would catch it in a moment. If the thread would not retain the virtue long enough, or if I wanted a leſs variable degree of electricity than the thread could retain, I had recourſe to the *feathers*, which never failed to retain the virtue that was communicated to them for ſeveral hours together. I have often found them pretty ſtrongly electrified, after remaining untouched a whole night, though there had been no fire in the room. They might be touched without any ſenſible loſs of their electricity; but they received the virtue very ſlowly.

The reader muſt obſerve, that by the *poſitive* or *negative* ſide of a tourmalin, in the following experiments, I always mean the ſide which is poſitive or negative while the ſtone is cooling. Alſo, when I mention *the tourmalin* without any diſtinction, I always mean Dr. Heberden's large one, the convex ſide of which is poſitive in cooling, and the flat ſide negative.

The conſideration of Mr. Wilcke's experiments on the production of ſpontaneous electricity, by melting one ſubſtance within another, firſt made me conjecture, that the tourmalin might collect its electricity from the neighbouring air. To aſcertain this circumſtance I made the following experiments, which ſeem to prove that my conjecture was juſt. It was with a view to this experiment that I firſt expreſſed a deſire to have a tourmalin in my poſſeſſion. I afterwards found that Mr. Wilſon had made an experiment, mentioned p. 296, which is, in part, favourable to this hypotheſis, though he ſuppoſed the electricity to permeate the ſtone, ſo that one ſide might have been ſupplied from the other. But the following experiments will ſhow, that the ſuppoſition of the permeability of the tourmalin to the electric fluid is altogether unneceſſary to account for any of the appearances it exhibits.

On the ſtandard bar of a moſt excellent pyrometer made by Mr. Ellicott, I laid a part of a pane of glaſs, and upon the glaſs Dr. Heberden's large tourmalin. The bar was heated by a ſpirit lamp placed underneath it; and I treated the tourmalin in this manner, to aſcertain with exactneſs when the heat was increaſing, decreaſing, or ſtationary. In this diſpoſition of my apparatus I obſerved, that, whenever I examined the tourmalin, the glaſs had acquired an electricity oppoſite to that of the ſide of the ſtone which had lain upon it, and equally ſtrong. If, for inſtance, I preſented the flat ſide of the ſtone to a feather electrified poſitively, as the heat was increaſing, it would repel it at the diſtance of about two inches, and the glaſs would attract it at the ſame, or a greater diſtance; and when the heat was decreaſing, the ſtone would attract it, and the glaſs repel it at the diſtance of four or five inches. It made no difference which ſide of the glaſs I preſented; both ſides attracting or repelling the ſame feather with equal ſtrength. When I faſtened a ſhilling with ſealing-wax upon the glaſs, the events were always the ſame. The electricity of both the ſhilling and the glaſs was always oppoſite to that of the ſtone. I was ſurpriſed to obſerve how ſoon the electricity, both of the ſtone and the glaſs, would change when it came to the turn; for in leſs than a minute I have ſometimes found them the reverſe of what they were before.

There was, however, in the caſes in which I laid the convex ſide of the tourmalin upon the flat ſurface of the glaſs, or ſhilling, one exception to the rule above mentioned, viz. that, in cooling, the glaſs and ſhilling were poſitive, as well as the ſtone. This I imagined to be owing to the ſtone touching the ſurface on which it lay in ſo few points, that it collected its electricity from the air, and imparted it to the body on which it lay; and this ſuppoſition was confirmed by experiment. For getting a mold made for the convex ſide of the ſtone in plaiſter of Paris, and heating the tourmalin in the mold, faſtened to a ſlip of glaſs, I always found the mold and the glaſs poſſeſſed of the electricity contrary to that of the ſtone, and equally ſtrong. When they were cooling, the mold ſeemed ſometimes to be more ſtrongly negative than the ſtone was poſitive; for, at one time, when the ſtone repelled the thread at the diſtance of about three inches, the mold attracted it at the diſtance of near ſix.

HAVING made the experiments above mentioned with the tourmalin placed upon glaſs, or conducting ſubſtances, laid upon the glaſs, I had the curioſity to try what would be the conſequence of heating and cooling the ſtone in contact with other ſubſtances, both electrics and conductors. And theſe experiments brought me gradually to the diſcovery of a method of reverſing all the experiments that have hitherto been made upon the tourmalin, making that ſide which is poſitive in heating or cooling to be negative, and that which is negative to be poſitive; ſo that the kind of electricity ſhall be juſt what the operator ſhall direct, by the application of proper ſubſtances to the ſtone.

I BEGAN theſe experiments with ſubſtituting another tourmalin inſtead of the piece of glaſs above mentioned; and when only one of the tourmalins was heated, they were both affected juſt as the tourmalin and glaſs had been. If, for inſtance, the negative ſide of a hot tourmalin was laid upon the negative ſide of a cold one, this latter became poſitive, as a piece of glaſs would have been in the ſame circumſtances.

WHEN I heated both the tourmalins, though they were faſtened together with cement, they both acquired the ſame power that they would have done in the open air. In theſe caſes, as the ſtones could not be made to touch one another in a ſufficient number of points, nothing could be concluded from the experiments. The ſame objection lay againſt heating or cooling the tourmalin upon rough glaſs; when I always found them both to be affected as they would have been if the glaſs had been ſmooth.

THIS conſideration made me think of cooling the tourmalin in contact with *ſealing-wax*, which might be made to fit the ſtone as exactly as poſſible, though it were ever ſo irregular. Accordingly I half buried the negative ſide of a tourmalin in hot ſealing-wax; and when it was cold, turning it out of its waxen cell, found it poſitive (contrary to what it would have been in the open air) and the wax negative. The other ſide of the tourmalin, which was expoſed to the open air, was affected in the ſame manner as it would have been if the oppoſite ſide had been expoſed to the air too, ſo that both ſides were poſitive in cooling. As the negative ſide of the tourmalin became poſitive by cooling in wax, I had no doubt but that the poſitive ſide would be ſo, as I actually found it.

I would have aſcertained the ſtate of the different ſides of the tourmalin when it was *heating* in wax, but I found it extremely difficult to do it with ſufficient certainty. It cannot be known exactly when the ſtone begins to cool in theſe circumſtances; beſides, in this method of treatment, it muſt neceſſarily be ſome time in the open air before it can be preſented to the electrometer; and the electricity of the ſides in heating is by no means ſo remarkable as it is in cooling. In the attempts I did make with the poſitive ſide of the tourmalin buried in wax, I generally found it negative, but once or twice it ſeemed to be poſitive.

When I cooled the tourmalin in *quickſilver*, contained in a china cup, it always came out poſitive, and left the quickſilver negative; but this effect could not be concluded to be the conſequence of the application of the one to the other, becauſe it is almoſt impoſſible to touch quickſilver with the tourmalin without ſome degree of friction; which never fails to make both ſides ſtrongly poſitive though it be quite cold, and eſpecially if the ſtone be dipped deep into it.

It then occurred to me, that the tourmalin would not be apt to receive any friction from ſimple preſſure againſt the palm of my hand; and this being a conducting ſubſtance communicating with the earth, the circumſtances of the experiment would be new, and might poſſibly produce new appearances. The event more than anſwered my expectations: for in heating or cooling the tourmalin in contact with the palm of my hand, each ſide of the ſtone was affected exactly in a manner contrary to what it would have been if expoſed to the open air. In this caſe, though the poſitive appearances may be ſuſpected to be ambiguous, on account of the difficulty of avoiding ſome ſmall degree of friction, in removing the ſtone from the hand; yet the negative appearances are, by that very circumſtance, rendered the more indiſputable, and therefore remove the objection from the poſitive ones. For the greater ſatisfaction of my reader, I ſhall relate theſe experiments exactly as they were made.

I FASTENED the convex ſide of Dr. Heberden's large tourmalin to the end of a ſtick of ſealing-wax, and when it was quite cold, I preſſed the flat ſide of it pretty hard againſt the ſofteſt part of the palm of my hand. Immediately upon this, preſenting it to an electrified feather, it appeared to be ſtrongly negative, contrary to what it would have been if expoſed to the open air; and it continued negative till it had acquired all the heat it could get from my hand, when its power decreaſed, though it was ſenſibly negative to the laſt. Perceiving no alteration, I let the ſtone cool in the open air; when, according to Mr. Canton's rule, it grew more ſtrongly negative, till it was quite cold. Thus the ſame ſide of the ſtone was made negative both in heating and cooling.

HEATING the ſame flat ſide, by holding it near a red-hot poker, and then juſt touching it with the palm of my hand (when I could not bear it to reſt a moment) it became poſitive. Letting it cool in the air, it was negative, and touching it again with my hand it became poſitive. Thus I made the ſame ſide of the ſtone alternately poſitive and negative for a conſiderable time; and at length, when I could bear to keep it upon my hand, it acquired a ſtrong poſitive electricity, which continued till it was brought to the heat of my hand.

To complete theſe experiments, I removed the wax from the convex ſide, and faſtened it to the flat ſide of the ſtone. Then warming the convex ſide, by preſſing it againſt the palm of my hand, it became pretty ſtrongly poſitive, contrary to what it would have been if heated in the open air, and continued poſitive in a ſmall degree after it had got all the heat it could from my hand. Letting it cool in the open air, it grew, according to Mr. Canton's rule, more ſtrongly poſitive, and continued ſo till it was quite cold. Thus the ſame ſide of the ſtone was made poſitive both in heating and cooling.

I then heated the convex ſide, by holding it near a red-hot poker, and preſſing it againſt the palm of my hand, as ſoon as I could bear it, it became (contrary to what it would have been in the open air) pretty ſtrongly negative; though it be extremely difficult to get a negative appearance from this ſide. It cannot always be catched when it is heating in the open air. Care, however, muſt be taken, leſt a ſlight attraction of the electrified feather, by a body not electrified, be miſtaken for negative electricity.

Having made the above mentioned experiments, to ſee how the tourmalin would be affected by being heated or cooled in contact with various ſubſtances, to which only one of its ſides was expoſed at once; I made others in which the ſtone was entirely ſurrounded by them. It appeared very evident, from Mr. Canton's experiment, that it could anſwer no purpoſe to incloſe it in ſubſtances that were conductors: for though the two electricities ſhould be generated, the equilibrium would inſtantly be reſtored between them. I therefore made uſe of electric ſubſtances only, and began with *oil* and *tallow*, both covering the tourmalin with them when it was hot, and alſo heating it in boiling oil. But this treatment produced no new appearance, the electricity of the ſtone being only a little leſſened. The event was the ſame when a tourmalin was covered with *cement* made of bees wax and turpentine.

AT laſt I made a ſmall tourmalin very hot, and dropping melted ſealing-wax upon it, covered the ſtone all over, to the thickneſs of about a crown piece; and found it to act nearly, if not quite as well through this coating of wax, as if it had been expoſed to the open air. I take it for granted, that the inſide of the caſe of wax next to the ſtone was poſſeſſed of the electricity oppoſite to that of the ſtone, at the ſame time that the outſide was the ſame with it. A pretty deception may be made by means of this experiment; for if a tourmalin be concealed in a ſtick of ſealing-wax, the wax will ſeem to have acquired the properties of the tourmalin.

HEATING the ſtone, or letting it cool *in vacuo* might eaſily be imagined to have the ſame effect as heating or cooling it in contact with conducting ſubſtances; I had the curioſity, however, to try the experiment, by letting it cool in an exhauſted receiver, in which I had a contrivance to bring a thread of trial near it, or withdraw it at pleaſure. The ſtone was ſet upright on its edge, by means of bits of glaſs which it touched but in a few points. The conſequence was, that the virtue of the ſtone ſeemed to be diminiſhed about one half; owing, perhaps, to the vacuum not being ſufficiently perfect. For the ſame reaſon, the tourmalin has but little virtue immediately upon being taken out of boiling water, or after being heated in flame.

ONE time I fixed a thin piece of glaſs, with a ſmall coating upon it, oppoſite and parallel to the flat ſide of the tourmalin, and at about a quarter of an inch diſtance from it, in an exhauſted receiver; to obſerve whether the electricity would be tranſmitted from the glaſs to the ſtone through the vacuum:

but though the glaſs was electrified, it was ſo ſlightly, that I could not be certain of what kind it was.

In order to aſcertain the circumſtances relating to the change of the electricity of the tourmalin with more exactneſs than could be done by heating and cooling the ſtone in any of the uſual methods, I laid it upon the ſtandard bar of the pyrometer, and communicated heat to it by a ſpirit lamp placed underneath it. The reſult of theſe experiments was in general agreeable to Mr. Canton's rules; but a few circumſtances occurred in this method of treating it, which could not be determined in any other; and therefore it may be worth while juſt to mention them. I generally heated the bar, which is of iron, eight inches long, till the index moved ſeventy degrees, each of which correſponds to one 7200th part of an inch; and obſerved, that which ever ſide of the ſtone lay uppermoſt, it was extremely difficult to aſcertain the nature of its electricity all the time the heat was increaſing; though, in order to do it, I held over it an electrified thread, about two inches in length, faſtened to a ſtick of ſealing-wax, which juſt ſupported it in an horizontal ſituation. It was evident, however, that it was electrified, by its attracting a thread of trial at the diſtance of about a quarter of an inch; but if I took the ſtone off the bar, and immediately preſented the ſide that had lain upon it to an electrified thread or feather, I always perceived the convex ſide to be negative, and the flat ſide poſitive in the ſame circumſtances; but not half ſo much as they were in the contrary ſtate by cooling. In this caſe, the two powers were very diſtinguiſhable by the ſmall thread above mentioned, as the ſtone lay upon the bar; and alſo by bits of down faſtened to ſilk threads. One of theſe, which had touched the convex ſide of the ſtone, as it lay uppermoſt upon the bar, could not be made to touch it again in leſs then five hours and a half.

To ſee what would be the effect of keeping the tourmalin in the very ſame degree of heat a conſiderable time together, I laid it upon the middle part of the bar, heated by two ſpirit lamps, one at each extremity, and making the index move forty-five degrees, I kept it in the ſame degree of heat, without the leaſt ſenſible variation, for above half an hour together; and obſerved, that the upper ſide, which was the convex one, was always electrified to a ſmall degree, attracting a fine thread at the diſtance of about a quarter of an inch. If in that time I took it off the bar ever ſo quick, and preſented it to an electrified feather; the flat ſide, which lay upon the bar, was negative, and the upper ſide very ſlightly poſitive; as appeared by its only not attracting the feather. When I put a piece of glaſs betwixt the ſtandard bar and the tourmalin, and kept them likewiſe in the ſame degree of heat, for the ſame ſpace of time, the reſult was the ſame as before, and the glaſs was ſlightly electrified, in a kind oppoſite to that of the ſtone.

In heating the tourmalin upon the pyrometer, one of its ſides was neceſſarily made much hotter than the other. This inconvenience I avoided in the following method of treatment, which, though not ſo accurate in ſome reſpects, has peculiar advantages in others. By means of two rough places in the ſtone, I tied it in a ſilk thread, which only touched the extreme edge of it on both ſides. Being in this manner perfectly inſulated, I contrived to make it hang in the air, at any diſtance from a fire, or candle, &c. and by twiſting the ſtring, I could make it preſent both its ſides alternately, ſo as to heat it very equally.

When, in this manner, I had made it ſo hot, that I could hardly bear to handle it, I let it remain in the ſame ſituation a quarter of an hour, in order to be ſure that it was heated equally throughout. Then, with a bundle of fine thread, held ſome time before in the ſame degree of heat, I took off the electricity which the ſtone had acquired in heating, and continuing it in the ſame ſituation, I found it acquired extremely little, if any electricity. Sometimes, when I thought it had acquired a little (which might be occaſioned by the variation of heat in the fire) it was ſo ſmall, that I could not determine of what kind it was. This fully ſatisfied me of the juſtneſs of Mr. Canton's obſervation that it is not heat, but the circumſtance of changing its degree of heat that gives electricity to this ſtone.

If the ſtone be heated pretty ſuddenly, I have ſometimes found that it may be handled, and preſſed with the fingers ſeveral times before the electricity it acquires in heating will be changed, though it begins to cool the moment it is removed from the fire.

In this ſame method of treatment, I verified Mr. Canton's obſervation, that when the tourmalin is heated, and ſuffered to cool again, without either of its ſides being touched, the ſame ſide will be poſitive or negative the whole time of the increaſe and decreaſe of the heat. But, as he obſerves, in his experiments on hot air, the ſtone muſt, in this caſe, be heated only to a ſmall degree. I alſo proved the converſe of this propoſition; for, beginning where I left the ſtone in the laſt experiment, and removing it farther from the fire, both ſides acquired a ſtrong electricity, as uſual; and bringing it again nearer to the fire, I obſerved that both the ſides not only retained the electricity they had acquired in cooling, all the time it was heating, but a conſiderable time after it had remained in the ſame degree of heat.

I cannot, however, entirely acquieſce in the reaſon that Mr. Canton gives for this appearance: for if the ſurrounding air would conduct the electric fluid from the poſitive ſide of the ſtone to the negative, I ſhould think it would be in the ſame ſituation as in the experiment Mr. Canton made upon it ſurrounded

rounded with water, and that neither ſide would diſcover any electricity at all. When the heat is three or four times greater than is ſufficient to change the electricity of the two ſides, the virtue of the ſtone is the ſtrongeſt, and appears to be ſo when it is tried in the very neighbourhood of the fire. In the very center of the fire, the ſtone never fails to cover itſelf with aſhes, attracted to it from all ſides, and from this property it acquired its name in Dutch.

It requires, indeed, ſome time for the electricity of the ſides to change from one ſtate to the other; and therefore the time of the ſenſible change is not always at the time of its beginning to cool, but theſe two circumſtances will be brought nearer together the hotter the ſtone is made, becauſe then the efforts (of whatever kind they are) to acquire any particular ſpecies of electricity will be the moſt vigorous, and ſooner produce their effect; ſo as to be more able to overcome obſtacles to it, ſuch as muſt ariſe from the contrary electricity with which the ſtone is poſſeſſed. Thus, if either ſide of the ſtone be in a ſtate to acquire either kind of electricity, and a quantity of the contrary electricity be communicated to it by friction or *ab extra*, that foreign electricity will be either only weakened, or loſt, or changed; and theſe in a longer or a ſhorter ſpace of time, according to the vigour, as we may ſay, with which the ſtone is made to exert itſelf to counteract that influence. But I have great reaſon to ſuſpect my own opinion, when it is different from that of ſo accurate and excellent a judge of this ſubject as Mr. Canton.

It is a fact, however, that the ſtone often changes its electricity very ſlowly; and the electricity it acquires in cooling never fails to remain many hours upon it, with very little diminution. It is even poſſible that, in ſome caſes, the electricity acquired by heating may be ſo ſtrong, as to overpower that which is acquired by cooling; ſo that both ſides may ſhow the ſame power in the whole operation. And I am very certain that, in my hands, both the ſides of Dr. Heberden's large tourmalin have frequently been poſitive for ſeveral hours together, without any appearance of either of them having been negative at all. Perhaps the flat ſide of this ſtone, which is poſitive in heating, might continue ſo according to Mr. Canton's obſervation, and the electricity of the convex ſide might have changed, as it very often does, too ſoon for me to obſerve it. This fact, however, has happened ſo often with me, and is ſo very remarkable, that I think I ought not to omit the mention of it, let the cauſe be what it will.

This appearance happened ſo conſtantly when I firſt began to make experiments with the tourmalin, that I had concluded the Duc de Noya had reaſon to aſſert, contrary to Æpinus, that both ſides of the tourmalin in all caſes acquired poſitive electricity; and I ſhould have acquieſced in that opinion, had it not been for the friendly remonſtrances of Dr. Franklin and Mr. Canton; in conſequence of which I renewed my experiments, and at length found other appearances. At the time above mentioned, I generally heated the tourmalin by preſenting each ſide alternately to a red-hot poker, or a piece of hot glaſs held at the diſtance of about half an inch; and ſometimes I held it in the focus of a burning mirror; but I have ſince found the ſame appearance when I have heated it in the middle of an iron hoop made red-hot. The ſtone, in all theſe caſes, was faſtened by its edge to a ſtick of ſealing-wax. This appearance I have obſerved to happen the ofteneſt when the iron hoop has been exceedingly hot, ſo that the outſide of the ſtone muſt have been heated ſome time before the inſide; and I alſo think there is the greateſt chance of producing this appearance when the convex ſide of the ſtone is made the hotter of the two. When I heat the large tourmalin in this manner, I ſeldom fail to make both ſides poſitive till the ſtone be about blood warm. I then generally obſerve a ragged part of the flat ſide, towards one end of the ſtone, will become negative firſt, and by degrees the reſt of the flat ſide; but very often one part of the flat ſide will, in this method of treatment, be ſtrongly poſitive half an hour after the other part is become negative.

THIS account of the appearance is made the more probable by the manner in which the ſtone was affected when only one of its ſides was heated at one time. For when the convex ſide only was heated, the ſtone often continued a long time with both its ſides poſitive, generally till it was not ſenſibly warm. But, in this caſe, before the convex ſide became poſitive, it would ſometimes be negative two or three minutes. On the other hand, when the flat ſide only was heated, it would be poſitive a long while, and the convex ſide negative; but the flat ſide becoming negative a conſiderable time before the convex ſide ceaſed to be ſo, both ſides would continue negative till the ſtone was nearly cold.

EXTREMELY ſorry I am for the article with which I muſt cloſe this ſection. In the firſt of the above mentioned courſes of experiments, that fine tourmalin, which has been ſo often mentioned in the courſe of this work, ſlipped out of my hands; and though it fell only from the height of my breaſt, upon a boarded floor, two pieces were broke off from one of its ends. The ſtone, however, is more disfigured than injured by the accident: for the larger of the fragments weighs but ten grains, and the ſmaller only one, while the reſt of it weighs four penny weights ſixteen grains. I cannot perceive that its virtue is at all leſſened. Mr. Wilſon obſerves, that there were ſeveral cracks in it; and for that reaſon I had been careful never to expoſe it to any great degree of heat.

It is broke with eight or ten different faces, each of which hath a moſt exquiſite poliſh; but there is no appearance of any *ſtrata* or *laminæ* in the internal ſtructure of the ſtone. A piece of glaſs or pitch might be ſuppoſed to break in the ſame manner. The larger of the fragments has conſiderable power, and the two ſides have the ſame different powers that they had when they were part of the intire ſtone.

SECTION XIII.

EXPERIMENTS IN WHICH RINGS, CONSISTING OF ALL THE PRISMATIC COLOURS, WERE MADE BY ELECTRICAL EXPLOSIONS ON THE SURFACES OF METALS.

IT was a diſcovery of Sir Iſaac Newton, that the colours of bodies depend upon the thickneſs of the fine plates which compoſe their ſurfaces. He hath ſhown that a change of the thickneſs of theſe plates occaſions a change in the colour of the body, rays of a different colour being thereby diſpoſed to be tranſmitted through it; and, conſequently, rays of a different colour reflected at the ſame place, ſo as to preſent an image of a different colour to the eye. A variation in the denſity occaſions a variation in the colour, but ſtill a medium of any denſity will exhibit all the colours, according to the thickneſs of it. Theſe obſervations he confirmed by experiments on plates of air, water, and glaſs. He likewiſe mentions the colours which ariſe on poliſhed ſteel by heating it, as likewiſe on bell metal, and ſome other metalline ſubſtances, when melted and poured on the ground, where they may cool in the open air, and he aſcribes them to the *ſcoriæ* or vitrified parts of the metal, which he ſays moſt metals, when heated or melted, do continually protrude,

trude, and ſend out to their ſurfaces, covering them in the form of a thin glaſſy ſkin.

This great diſcovery concerning the colours of bodies, depending upon the thickneſs of the fine plates which compoſe their ſurfaces, of whatever denſity thoſe plates may be, I have been ſo happy as to hit upon a method of illuſtrating and confirming by means of electrical exploſions. A number of theſe being received on the ſurface of any piece of metal, change the colour of it to a conſiderable diſtance from the ſpot on which they were diſcharged, ſo that the whole circular ſpace is divided into a number of concentric rings, each of which conſiſts of all the priſmatic colours, and perhaps as vivid as they can be produced in any method whatever.

It was not by any reaſoning *a priori*, but by mere accident, that I was led to the diſcovery of theſe colours. Having occaſion to take a great number of exploſions, in order to aſcertain the lateral force of them; I obſerved that a piece of braſs, through which they were tranſmitted, was not only melted, and marked with a circle by a fuſion round the central ſpot, but likewiſe tinged beyond the circular ſpot with a greeniſh colour, which I could not eaſily wipe out with my finger. Struck with this new appearance, I replaced the apparatus, and continued the exploſions; till, examining with a microſcope, I plainly perceived all the priſmatic colours, in the order of the rainbow. The diameter of the red, in this inſtance, was one third of an inch, and of the purple near one fourth. The diameter of the whole coloured ſpace in the ſubſequent experiments, in which I generally uſed thirty or forty exploſions, was near an inch.

Pleased with the firſt experiment, I preſently diverſified it in a great variety of ways, the reſult of which I ſhall compriſe in the following obſervations.

1. When a pointed body is fixed oppoſite to a plain ſurface, the nearer it is placed, the ſooner the colours appear, the cloſer do they ſucceed one another, and the leſs ſpace they occupy. It ſeems, however, that when the point is at ſuch a diſtance, that the electric matter has room to expand, and form as large a circular ſpot as the battery will admit, this coloured ſpace is as large as it is capable of being made; but ſtill the colours appear later, in proportion to the diſtance beyond that. When the point is fixed exceedingly near, or made to touch the ſurface, the colours appear at the firſt exploſion, but they ſpread very irregularly, and make no diſtinct rings.

2. The more accutely pointed is the wire, from which the electric fire iſſues, or at which it enters, the greater is the number of rings. A blunt point makes the rings larger, but fewer. It is likewiſe much later before they make their appearance at a given diſtance.

3. In making theſe rings, the firſt appearance is a duſky red about the edge of the central ſpot; preſently after which (generally after four or five ſtrokes) there appears a circular ſpace, viſible only in an oblique poſition to the light, and looking like a ſhade on the metal. This expands very little during the whole courſe of the exploſions. It ſeems to be an attempt, as it were, at the firſt red; for, by degrees, as the other colours fill the greater part of that ſpace, the extreme edge of it becomes a deeper brown.

4. After a few more exploſions, ſecond circular ſpace is marked, by another ſhade beyond the firſt, being one eighth or one tenth of an inch in breadth, which I have never obſerved to change its appearance, after ever ſo many exploſions. This ſhade, by ſucceeding the firſt, which becomes gradually of a brown or light red colour, ſeems to be an attempt at the fainter colours, which intervene between the reds.

5. All the colours make their firſt appearance about the edges of the circular ſpot. More exploſions make them expand towards the extremity of the ſpace firſt marked out; while others ſucceed in their place; till, after thirty or forty exploſions, three diſtinct rings appear, each conſiſting of all the colours. If the the exploſions be continued farther, the colours become leſs beautiful and diſtinct, the red generally prevailing, and ſuffuſing the reſt.

6. The innermoſt, *i. e.* the laſt formed colours, are always the moſt vivid, and thoſe rings are likewiſe cloſer to one another than the reſt.

7. These colours may be bruſhed with a feather, or a finger may be drawn over them, without injury; but they are eaſily peeled off, with one's nail, or any thing that is ſharp. The innermoſt are the moſt difficult to eraſe.

8. The firſt rings are ſometimes covered with a quantity of black duſt, part of which, however, may be wiped off, with a feather, and the colours will appear under it.

9. It makes no difference whether the electric matter iſſue from the pointed body upon the plate, or from the plate upon the pointed body, the ſurface oppoſite to the point being marked exactly alike in both caſes; alſo the points themſelves, from which the fire iſſues, or at which it enters, are coloured to a conſiderable degree, about half an inch. The colours, alſo, return here as upon the plate.

10. The more circles are made at the ſame time, the more delicate, I think, the colours will be, whereas the ſurface is torn, as it were, by violent exploſions, and the colours appear rough and coarſe. But this roughneſs is only perceived on ſteel. On ſilver, tin, and poliſhed braſs, the colours were always equally free from that coarſeneſs.

11. A POLISHED ſurface is not neceſſary for theſe colours, for they appear very well, though they do not make ſo beautiful an appearance on the rough ſurfaces.

12. THESE coloured rings appear equally well on all the metals that I have tried, viz. gold, ſilver, copper, braſs, iron, ſteel, lead, and tin. I have not tried any of the ſemi-metals, but I have no doubt of their anſwering as well as the proper metals.

13. WHEN the pointed wire was made to incline to the plane on which the colours were made, the circular ſpot was quite round, and the center of it was in a perpendicular let fall from the point upon the plain ſurface; but the colours were projected in an oblong form, the center being in the pointed wire continued.

UPON ſhewing theſe coloured rings to Mr. Canton, I was agreeably ſurprized to find, that he had likewiſe produced all the priſmatic colours from all the metals, but by a different operation. He extended fine wires, of all the metals, along the ſurface of pieces of glaſs, ivory, wood, &c. and when the wire was exploded, he always found them tinged with all the colours. They are not diſpoſed in ſo regular and beautiful a manner as in the rings I produced, but they equally demonſtrate, that none of the metals, thus exploded, diſcovers the leaſt preference to one colour more than to another. A variety of other very extraordinary appearances occurred in the courſe of Mr. Canton's experiments in melting wires, but I forbear to mention them, as I hope he will ſoon favour the public with a communication of them himſelf.

IN what manner theſe colours are formed, it may not be eaſy to conjecture. In Mr. Canton's method of producing them, the metal, or the calcined and vitrified parts of it, ſeem to be diſperſed in all directions from the place of exploſion in the form of ſpheres, of a very great variety of ſizes, tinged with all the variety of colours, and ſome of them ſmaller than can be diſtinctly ſeen by any magnifier. In my method of making theſe colours, they ſeem to be produced in a manner ſimilar to the production of colours on ſteel, and other metals by heat, *i. e.* the ſurface is affected, without the parts of it being removed from their places, certain plates or *laminæ* being formed, of a thickneſs proper to exhibit the reſpective colours.

SECTION XIV.

EXPERIMENTS ON THE LATERAL FORCE OF ELECTRICAL EXPLOSIONS.

BEING informed, in accounts of damages done by lightning, of perſons and things being removed to a conſiderable diſtance, without receiving any hurt; I was excited to try whether I could produce ſimilar effects by electricity. All the other known effects of lightning had been frequently imitated by the application of this power, but I do not know that this effect has ever been ſo much as taken notice of by any electricians. The experiments I preſently found to be very eaſy, and I think it not difficult to aſcertain the cauſe, and the manner in which this ſtriking effect is produced.

IF pieces of cork, powder of any kind, or any light bodies whatever, be placed near the exploſion of a jar or battery, they will not fail to be moved out of their places, upon the inſtant of the diſcharge. If the exploſion of a large battery be made to paſs over the ſurface of animal or vegetable ſubſtances, in the manner deſcribed above, and large corks be ſtrewed along or near the part intended for it, it is ſurprizing to obſerve with what violence they will be driven about the room. This diſperſion is in all directions from the center of the exploſion, and it makes no difference whether the rods, between which it is made, be ſharp pointed or otherwiſe.

The effect of this lateral force is very remarkable in attempts to fire gun powder in electrical exploſions. If the gun powder be confined ever ſo cloſe in quills or cartridges, and they be held faſt in viſes; yet, when the exploſion is made in the center of them, it will ſometimes happen (even when a wire has been melted in the midſt of the powder, and the fragments have been ſeen red-hot, for ſome time, in different parts of the room] that the powder has not been fired, or only a few grains of it, the reſt being diſperſed with great violence, part of it flying againſt the faces of perſons who aſſiſted in making the experiments. This circumſtance, together with the charcoal being a conductor of electricity makes it ſo extremely difficult to fire gun powder by electrical exploſions; and it is evidently owing to this lateral force, that parts of the melted wire fly ſo many ways, and to ſo great a diſtance from the place of exploſion.

This lateral force is exerted, not only in the neighbourhood of an exploſion, when it is made between pieces of metal in the open air, but alſo when it is tranſmitted through wires that are not thick enough to conduct it perfectly; and the ſmaller the wire, and the more complete the fuſion, the greater is the diſperſion of light bodies placed near it. At one time, when the wire was not melted, but turned blue by the exploſion (in which caſe it generally aſſumes a duſky red, which laſts but for a moment) there was a ſmall diſperſion from every part of the wire, but by no means ſo great as it would have been if it had been melted, or only heated to a greater degree.

By a conſiderable number of trials, I found, that a greater force of exploſion would move light bodies at a greater diſtance, but the ſmaller the bodies were, the leſs was this difference; ſo that I imagined, that if they had no weight at all, they would, probably, be moved at the ſame diſtance, by the exploſion from any quantity of coated ſurface, charged equally high: but there was a great difference in the weight removed by different forces at the ſame diſtance. Placing the ſame piece of cork at the ſame diſtance from the place of exploſion, I found that the diſcharge of one jar removed it one fourth of an inch, two jars one inch and one fourth, three jars one inch and three fourths, and four about two inches, ſo that I do not wonder at very heavy bodies being moved from their places, and to conſiderable diſtances, by ſtrong flaſhes of lightning.

THAT the immediate cauſe of this diſperſion of bodies in the neighbourhood of electrical exploſions is not their being ſuddenly charged with a quantity of electric matter, and therefore flying from others that are equally charged with it is, I think, evident from the following experiments and obſervations. I never obſerved the leaſt ſenſible attraction of theſe light bodies to the braſs rods through which the exploſion paſſed, or to the electric matter paſſing between them, previous to this repulſion, though I uſed ſeveral methods, which could not have failed to ſhow it, if there had been any ſuch thing. Sometimes I ſuſpended them in fine ſilken ſtrings, and obſerved that they had contracted no electricity after they had been agitated in the manner deſcribed above. Sometimes I dipped them in turpentine, and obſerved that no part of it was found ſticking, either to the braſs rods themſelves, or to any part of the table betwixt them and the place where the light bodies had been laid. I even found that the exploſion of a battery, made ever ſo near to a braſs rod, did not ſo much as diſturb the equilibrium of the electric fluid in the body itſelf; for when I had inſulated the rod, and hung a pair of pith balls on the end oppoſite to that near which the exploſion paſſed, I found that the balls were not in the leaſt moved at the time of exploſion; which they would have been, if part of the electric fluid natural to the body had been driven, though but for a moment, towards the oppoſite end. I alſo obſerved that the effect was the ſame when the exploſion was made to paſs through one of the knobs of the inſulated rod. This lateral force was evident through thin ſubſtances of various kinds, interpoſed between the exploſion and the bodies removed by it; as paper, tinfoil, and even glaſs; for when ſome grains of gun powder were put into a thin phial cloſe ſtopped, and held near the exploſion of a battery, they were thrown into manifeſt agitation.

I THEREFORE think it moſt probable, that this lateral force is produced by the exploſion of the air from the place where the exploſion is made. For the electric matter makes a vacuum of air in its paſſage; and this air, being diſplaced ſuddenly, gives a concuſſion to all the bodies that happened to be near it. Hence the removal of the light bodies, and the agitation communicated to the thin ſubſtance and to the air, and the light bodies placed beyond them.

THE only objection to this hypotheſis is, that this lateral force is not ſo much leſs in vacuo as might be expected, when the air is ſuppoſed to receive the concuſſion firſt, and to communicate it to other bodies; but it muſt be conſidered, that the moſt perfect vacuum we can make with a pump is not free from air. I have tried to make this experiment in a Torricellian vacuum, but could not ſucceed at that time. Beſides, as the electric matter of which an exploſion conſiſts muſt take a wider path in vacuo, if not equally fill the whole ſpace, it may affect a body in its paſſage, without the intervention of any air. In condenſed air, this latter force was not, as far as I could perceive, much encreaſed.

Willing to feel what kind of an impulſe it was that acted upon bodies, when they were driven away by this lateral force of electricity, I held my finger near the path of an exploſion of the battery, paſſing over the ſurface of a green leaf, when I felt a ſtroke, as of ſomething puſhing againſt my finger. Several corks, placed in the ſame ſituation, were driven to a conſiderable diſtance by the ſame exploſion.

Recollecting that this power, which I now call the *lateral force of electrical exploſions*, muſt be the ſame with that which gives the concuſſion to water, mentioned in my experiments to imitate an earthquake, and to vegetable and animal ſubſtances, over the ſurface of which it paſſes; and being determined to make a more ſatisfactory trial of it than I had ventured to do before, I laid a green leaf upon the palm of my hand, intending to make the exploſion paſs over the leaf; but the leaf was burſt, and torn to pieces, and the exploſion paſſing over my hand gave it a violent jar, the effect of which remained in a kind of tingling for ſome time.

Lastly, in order to judge the more perfectly of this force, I laid a chain communicating with the outſide of the battery upon my bare arm above the wriſt, and bringing the diſcharging rod near the fleſh, within about two inches and an half of the chain, I made the exploſion paſs over that quantity of the ſurface of the ſkin. Had I taken a greater diſtance, I was aware that the exploſion would have entered the fleſh, which, I was ſenſible, would have given a painful convulſion to the muſcles through which it paſſed. In this caſe, the ſenſible effect was very different from that, being the ſame external *concuſſion* as before, and I have ſometimes thought, that the ſenſation is not diſagreeable. However the hairs upon the ſkin were all ſinged, and curled up along the whole path of the exploſion, and for the ſpace of about half an inch on each ſide of it alſo the *papillæ pyramidales* of the ſkin were raiſed, as when a perſon is ſhivering with cold. This was alſo the caſe in every part of the arm which the chain touched, and even that part of it which was not in the circuit. Both the path of the exploſion, and the place on which the chain had lain, had a redneſs, which remained till the next day. Sometimes the fleſh has contracted a blackneſs by this experiment, which has remained for a few hours.

SECTION XV.

VARIOUS EXPERIMENTS ON THE FORCE OF ELECTRICAL EXPLOSIONS.

MAKING the exploſion of a battery paſs over the ſurface of a green cabbage leaf, I obſerved that it left a track, near a quarter of an inch in breadth, exceedingly well defined, and diſtinguiſhable by a difference of colour from the reſt of the leaf. Along this path alſo the firmneſs of texture in the leaf was entirely deſtroyed, that part becoming quite flexible, like a piece of cloth. Preſently after it turned yellow, grew withered, and became perfectly brittle.

WILLING to try the effect of this exploſion paſſing along the ſurface of other ſubſtances, I laid a piece of common window glaſs on the path, preſſed by a weight of ſix ounces; but it was ſhattered to pieces, and totally diſperſed, together with the leaf on which it lay. Placing the black ſide of a piece of cork wood upon it, preſſed by a weight of half a pound, the leaf was not rent, but the cork was furrowed all the way, a trench being made in it, about half an inch in breadth, and a quarter of an inch in depth. Laying the ſmooth cut ſurface of the piece of cork, it was furrowed all the way as if it had been cut with a file, but not near ſo deep as before. Many of the ſmall pieces which had been rubbed off in the exploſion, remained in the furrow. Alſo the ſubſtance of the cork ſeemed to be ſhattered, and it was eaſily rubbed off, a little way into it.

I MADE this exploſion on the ſurface of ſome red wine in a ſmall diſh, and kept a part of the ſame quantity expoſed in a ſimilar manner, but I could perceive no difference between them after ſeveral days.

THE track of an electrical exploſion on the ſurface of the cabbage leaf being ſo well defined, ſuggeſted an experiment to aſcertain whether there was any ſenſible *momentum* in the electric fluid, when it is ruſhing with violence from one ſide of a battery to the other. For this purpoſe I made the exploſion paſs over the leaves when they were cut in right and acute angles; ſo that the ſhorteſt path, from the inſide to the outſide of the battery, was to turn cloſe at the angle; and obſerved, that it was not diverted from its courſe in the leaſt degree by the rapidity of its own motion, but that it had turned exactly at the angle; and kept as cloſe to the oppoſite ſide, as if the motion had begun at the angle. The electric matter had however been evidently attracted by the veins of the cabbage leaf, having purſued them a little way, at leaſt having ſenſibly affected them, wherever it met with them in its paſſage.

THIS experiment ſuggeſted another, intended to determine whether the force of an exploſion was at all diminiſhed by being diverted from a right lined courſe, and made to turn in a great number of angles. To do this, I firſt found, by a great number of trials, what length of a ſmall iron wire I was able to melt with a battery of about twenty ſquare feet, in the middle of a circuit of about three yards of braſs wire, conſiderably thicker than the iron, and ſtretched in two right lines, ſuſpended on ſilken ſtrings. The length of the iron wire melted ed in theſe circumſtances was about three inches. I then took the ſame braſs wire, and, fixing pins into a board of baked wood, twiſted it about them, making it turn in a very great number of acute angles; and I put three inches of the ſame iron wire in the middle of this crooked circuit, that I had done in the ſtraight one, ſo that the electric matter in the exploſion was obliged to make a great number of turns at acute angles, before it could come to the iron wire; but I always found that the ſame length of iron wire was melted in theſe circumſtances, as in the other, and not the leaſt difference was perceived in the force.

BUT though the *form* of the wire through which an exploſion paſſed made no difference in its force, I found a very remarkable difference occaſioned by the *length of the circuit*, in wires of the ſame thickneſs, and which, I own ſurprized me very much.

IN order to aſcertain the practicability of firing mines by electrical exploſions, I took twenty-two yards of ſmall braſs wire (but ſo thick, however, that I could not have melted the leaſt part of it, by the force of any battery I have ever conſtructed) and extending it along a dry boarded floor, with a ſmall piece of iron wire, and a cartridge of gun powder about it, in the place that was moſt remote from the battery; I found that, upon the diſcharge, the wire was not melted, nor the gun powder exploded; alſo the report was very faint. In other circumſtances a charge of the ſame battery was able to melt more than nine inches of this iron wire, and this ſame cartridge was eaſily fired near the battery, connected with ſhorter pieces of the ſame braſs wire; ſo that the diminution of force muſt have been owing to the length of the circuit.

IN the place of this ſmall braſs wire, I ſubſtituted an iron wire, one fifth of an inch thick; when about half an inch of the ſmall iron wire was exploded; ſo that the force was not leſſened ſo much in a circuit of the thick iron wire, as it had been in one of the ſmall braſs wire. In order to judge how much of the force might be loſt by nearer circuits, conſiſting of leſs perfect conductors, I joined the middle of the circuit made by the iron wire with water, in which both the wires were immerſed. The effect was, that the ſmall iron wire was only made red-hot, but not exploded as before.

Being ſenſible how much depended upon avoiding all leſſer circuits, whereby part of the fire of an exploſion might return to the battery, without reaching the extremity of the circuit, where I intended the whole of its force to be exerted; in the remaining experiments, I inſulated half the circuit of iron wire. There was no occaſion for inſulating the whole circuit: for if there was but one paſſage *to* or *from* the middle of it, there could be put one *from* or *to* it. In this method it was eaſy to aſcertain what loſs of force was occaſioned by the length of the circuit, as every other circumſtance was carefully excluded. And it preſently appeared to be very conſiderable; for though I could melt nine inches of the ſmall iron wire at the diſtance of fifteen yards from the battery, when I tried twenty yards, I found that I was juſt able to make ſix inches of it red-hot. The battery in theſe experiments was in the houſe, and the wires of which the circuit conſiſted were conveyed by ſilken ſtrings into a garden adjoining to the houſe.

Mentioning this loſs of force, occaſioned by the length of the circuit, in electrical exploſions to Dr. Franklin, he told me that the ſame obſervation had occurred to him, and that he had alſo been diſappointed in an attempt to fire gun powder at a diſtance from his battery.

Struck with this appearance, I endeavoured to aſcertain the quantity of this obſtruction, by trying what other courſes the electric fire would chuſe, preferably to a long metallic circuit. In the firſt place, taking about a yard of the ſmall braſs wire mentioned above, I diſpoſed it in the manner deſcribed [fig. 9. Pl. I.] connecting one of the ends with the outſide of the battery and the other with the inſide. In the firſt place, I brought the parts [*a*] and [*b*] (near the two extremities) into contact, and, upon the diſcharge, found there had been a fuſion in that place, and that a great part of the fire had taken the ſhorter circuit, though it had been obliged to quit the wire in one place, and enter in again in another. Afterwards I removed the parts [*a*] and [*b*] to a ſmall diſtance from one another, and, upon the exploſion, obſerved a ſtrong ſpark paſs between them. Removing them to greater and greater diſtances, I found the exploſion choſe to paſs above one third of an inch in the air, rather than make the circuit of the continued wire. Uſing a longer and ſmaller iron wire, the paſſage through the air exceeded half an inch. I then took four or five yards of iron wire, one tenth of an inch thick; when the paſſage through the air was ſtill half an inch; and taking three yards and a half of a wire that was one fifth of an inch thick, the ſpark in the air was half an inch, and ſometimes near three quarters of an inch. Making uſe of only half the length of this wire, the paſſage through the air was only half that diſtance, or one fourth of an inch. When I kept the place of near contact about the middle of this wire, and made the exploſion at the extremities of the whole wire, I was obliged to bring them about as near again, i. e. to little more than one eighth of an inch before the paſſage would be through the air; ſo that the force of the whole exploſion muſt have been greatly weakened by its paſſing through ſo much of the wire. Laſtly, I took a pair of kitchen tongs, the legs of which were two feet in length, and the ſmalleſt part of them above half an inch in diameter; when the circuit was made about one ſixth of an inch in the air (for at that diſtance from one another the ends of the tongs had been fixed) rather than through four feet of that thick iron.

NOTWITHSTANDING this paſſage of the exploſion through the air, at the ſame time that a metallic circuit was open for it, it was evident that the whole of the force did not paſs this way, nor indeed the greateſt part of it. For when I extended a ſmall iron wire between [*a*] and [*b*], I could only make about half an inch of it red-hot, whereas, when there was no other metallic circuit, I was able, with the ſame battery, to explode more than two inches of it.

As the electric fire meets with ſo much obſtruction in paſſing through a circuit of iron of this thickneſs, I make no doubt, but that it is conſiderably obſtructed in paſſing through metallic circuits of any thickneſs whatever; and that it would prefer a paſſage through the air, if they were made even of no great length. In this method, the different degrees of conducting power in different metals may be tried, uſing metallic circuits of the ſame length and thickneſs, and obſerving the difference of the paſſage through the air in each. N. B. A common jar anſwers as well in theſe experiments as a large battery.

IT is evident, from many experiments, that the whole fire of an exploſion does not paſs in the ſhorteſt and beſt circuit, but that if inferior circuits be open, part will paſs in them at the ſame time. Of this I made the following ſatisfactory trial. I took an iron chain, and laid it upon a table, in contact with a charged jar; ſo that the parts of it made two circuits for the diſcharge, which I could vary at pleaſure; and I obſerved that, when one of the circuits was but half an inch, and the other more than half a yard; yet, if the charge was high, it always went in them both, there being conſiderable flaſhes between the links of the remoteſt part of the chain. If the charge was weak, it paſſed in the longer or metallic circuit only.

IT is evident that when the wires of a battery are not in cloſe contact, there muſt be ſome loſs of force in the diſcharge; but this never appeared to me to be ſo conſiderable, as Mr. L. Epinaſſe ſeems to have imagined *. In order to aſcertain this by experiment, I firſt found, by repeated trials, what length of an iron wire, of a certain thickneſs, I was able to melt with a battery conſiſting of twenty jars, conſtructed in the manner deſcribed above. It was about two inches and a half. I then ſoldered all the wires together, and alſo ſoldered one rod to them all, inſtead of a chain which I had uſed before, ſo that I avoided near a hundred ſparks, in each of which ſome force had been loſt; but I did not find, after many trials, that the power of the battery was ſenſibly diminiſhed. I ſtill could not melt three inches of the ſame wire.

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* Phil. Tranſ. Vol. lvii. pt. i. p. 186.

SECTION XVI.

MISCELLANOUS EXPERIMENTS.

I. OBSERVATIONS ON THE ELECTRIC SPARK TAKEN THROUGH SEVERAL PIECES OF METAL.

MARCH the 24th, 1766. I obſerved that an electric ſpark taken from the prime conductor itſelf was not near ſo ſtrong and pungent, as one taken through a piece of metal inſulated, and interpoſed between my finger and the conductor.

THE effect was the ſame whatever was the form of the interpoſed piece of metal. And, in this manner, whatever was preſented received a full and ſtrong ſpark; whereas a great part is commonly diſſipated, in pencils or ſtars, even when pretty large braſs knobs are preſented to the prime conductor itſelf, if the excitation be very powerful; unleſs both the conductor and knob have one preciſe degree of convexity, adapted to one another.

ONE ſingle braſs ball made the ſpark as ſtrong as the interpoſition of a long piece of metal, or of many pieces.

Whether one, a few, or a great number of pieces were uſed, it ſeemed that the intervals taken together muſt be equal.

But theſe intervals taken together will be larger when the pieces are placed in a right line, than when they are laid in a curve.

Whether one body, or a number of them be interpoſed; if a ſpark be ſolicited it will not ſtrike the firſt, unleſs it can, at the ſame time, ſtrike all the reſt.

All theſe experiments ſucceed, in the ſame manner, with the exploſion of a charged jar.

Some of the above mentioned circumſtances, I afterwards found, had been taken notice of by Signior Beccaria.

II. A DECEPTION RELATING TO THE DIRECTION OF THE ELECTRIC SPARK.

As I was once amuſing myſelf with taking long ſparks from a large prime conductor of poliſhed copper, and conſidering the deceptions that electricians had fallen into with reſpect to the direction of the electric matter; I could not help being ſtruck with one deception, which the evidence of my ſenſes would never have rectified; and which ſhowed very clearly, how little the evidence of the ſenſes is to be depended upon in ſuch caſes. I obſerved, that, whether I made this large conductor give, or take the electric fire (for I could make it do either at pleaſure, and with the ſame force) I ſtill fancied that a ſpark taken with a braſs ball above the conductor deſcended to it, and that a ſpark taken below it aſcended from it; but ſparks taken laterally ſeemed to have no one certain direction.

III. An experiment intended to aſcertain whether electric ſubſtances, in their natural ſtate, contain more of the electric fluid than conductors.

Thinking to aſcertain Dr. Franklin's hypotheſis, concerning the eſſential difference between conductors and non-conductors, I made a pretty large piece of glaſs red-hot (in which ſtate I had proved it to be a real conductor of electricity) and placed it upon a ſmooth piece of copper, inſulated; ſuppoſing that, if electric ſubſtances had naturally a much greater ſhare of the electric fluid than conductors, this piece of glaſs, in paſſing from a conducting to a non-conducting ſtate, muſt exhauſt the copper of its natural ſhare of the electric fluid, and leave it electrified negatively. But I could perceive no kind of electricity, either in the copper, or the glaſs, during the whole time of its cooling.

Some time after, I found that Mr. Cigna had endeavoured to aſcertain the ſame thing, by reducing ice into water; but ice and water are both conductors of electricity.

IV. The MUSICAL TONE of various discharges aſcertained.

As the courſe of my experiments has required a great variety of electrical exploſions, I could not help obſerving a great variety in the muſical tone made by the reports. This excited my curioſity to attempt to reduce this variation to ſome meaſure. Accordingly, November the 17th, by the help of a couple of ſpinets, and two perſons who had good ears for muſic, I endeavoured to aſcertain the tone of ſome electric exploſions; and obſerved, that every diſcharge made ſeveral ſtrings, particularly thoſe that were chords to one another, to vibrate: but one note was always predominant, and ſounded after the reſt. As every exploſion was repeated ſeveral times, and three of us ſeparately took the ſame note, there remained no doubt but that the tone we fixed upon was, at leaſt, very near the true one. The reſult was as follows.

A JAR containing half a ſquare foot of coated glaſs ſounded F ſharp, concert pitch. Another jar of a different form, but equal ſurface, ſounded the ſame.

A JAR of three ſquare feet ſounded C, below F ſharp. A battery, conſiſting of ſixty-four jars, each containing half a ſquare foot, ſounded F below the C.

THE ſame battery, in conjunction with another of thirty-one jars, each containing a ſquare foot, ſounded C ſharp. So that a greater quantity of coated glaſs, always gave a deeper note.

DIFFERENCES in the degree of a charge in the ſame jar made little or no difference in the tone of the exploſion; if any, a higher charge gave rather a deeper note.

FROM theſe experiments it will be eaſy for any perſon to compare the quantity of ſquare feet of coated glaſs, with the lengths of muſical ſtrings giving the ſame note. For this purpoſe, I could eaſily have found more terms of the ſeries; but I am afraid philoſophers in general will think it trifling enough to have found ſo many. I do not expect that electrical exploſions will ever be introduced into concerts of muſic; or that theſe experiments will be of any uſe to meaſure the extent of the clouds from which a clap of thunder proceeds. But true philoſophers will not abſolutely deſpiſe any new fact or obſervation, though it have no immediate, or apparent uſe.

V. Ex-

V. EXPERIMENTS ON THE EFFECTS OF GIVING A METALLIC TINGE TO THE SURFACE OF GLASS.

IT has long been a queſtion among electricians, where the electric matter that conſtitutes the charge of a plate of glaſs lies; whether within the pores of the glaſs, or only upon the ſurface; and ſome experiments I have made will perhaps be thought to throw ſome light on this difficult ſubject.

I CONSIDERED that the common coating of a jar is not in actual contact with the glaſs, but that the metallic tinge, which is given to glaſs by an electric exploſion of the metal upon its ſurface, is probably in contact with it, if not lodged in its pores. I therefore gave a coating of this kind to both ſides of a plate of glaſs; and at firſt imagined that the glaſs coated in this manner did receive a charge, as well as if it had been coated in the common way; for it gave a real ſhock; but I very well remember, at that time, being a little ſurpriſed to ſee the electric fire run over the ſurface of that coating, a thing not poſſible in the common way. However, not ſufficiently attending to that circumſtance, I was purſuing the experiment, and trying whether, by combining this piece of glaſs with a large battery, and making the diſcharge of both upon this metallic tinge, I could not melt part of it, and thereby fetch it out of the glaſs; as that method would have melted, and abſolutely diſperſed a conſiderable part of the coating of a common jar. But I was prodigiouſly ſurpriſed to find, that, though the connection of this metallic tinge with the battery was complete, the diſcharge could not be made by bringing the diſcharging rod upon it; though within three quarters of an inch of another braſs rod, that formed the communication between this plate of glaſs and the battery. This convinced me that the metallic tinge did not anſwer the purpoſe of a coating; and I preſently ſatisfied myſelf, that a piece of uncoated glaſs would receive juſt ſuch a charge as the tinged glaſs had done.

To aſcertain this matter ſtill farther, I ſtruck a tinge of this kind along two oppoſite ſides of a glaſs tube, about half a yard in length; and holding it with my hand in contact with a part of this metallic tinge, found that it was excited juſt like another tube: for when I diſcharged the electricity of any part of the tube where the tinge was ſtruck, it did not at all diſcharge other parts of the tube, whither the ſame tinge extended. Alſo the electric ſnapping from the tinged part of the glaſs could not be diſtinguiſhed from the ſnapping at other places; except that, ſometimes, where the gold lay thicker than ordinary, a denſer ſtream of electric matter was viſible on its ſurface, and ran in ſeveral ſmall ſtreaks, in different directions, from the place where the ſpark was taken.

THIS experiment ſeems to ſhow, that a coating of metal exceedingly near the ſurface of the glaſs is not at all affected either by the excitation or charging of it; and ſeems to confirm the hypotheſis of the electric fluid not entering the pores of the glaſs.

As the giving this metallic tinge to both ſides of a plate of glaſs is not very eaſy, the reader will not, perhaps, be diſpleaſed to be informed in what manner I ſucceeded in it. After fatiguing myſelf a long time in endeavouring to ſtrike a piece of leaf braſs into the two ſides of a plate of glaſs, to ſerve inſtead of a coating (having always broken the glaſs in fixing either the firſt or ſecond coating) I at length put two other pieces of glaſs, one on each ſide of that to which I intended to give the tinge, with pieces of leaf braſs between them both; and making one exploſion through both of them at the ſame time, the upper and the lower piece of glaſs were ſhattered to pieces, but the middle piece (being equally affected on both ſides) remained whole, and the coatings were nearly as I wiſhed them.

VI. An experiment intended to ascertain whether FERMENTATION contributes to the production of electricity.

September the 3d. In order to determine whether any of the electric fluid was diſcharged from, or acquired by bodies in a ſtate of *fermentation*; I hung a pair of pith balls at the extremity of a piece of wire communicating with a quantity of ſteel filings, fermenting with oil of vitriol, incloſed in a glaſs veſſel. But they never ſeparated in the leaſt.

VII. An experiment intended to ascertain whether EVAPORATION contributes to the production of electricity.

December the 26th. I put a ſmall quantity of water upon a thin piece of glaſs, and made it all ſuddenly evaporate by a red-hot iron held under it; but the glaſs had acquired no degree of electricity. The weather was froſty.

VIII. An experiment intended to ascertain whether FREEZING be accelerated or retarded by electrification.

January the 6th, 1767. I expoſed two diſhes of water in the open air, while it was freezing intenſely, and electrified one of them pretty ſtrongly; but could perceive no difference in the time, either when it began to freeze, which was in about three minutes, or in the thickneſs of the ice when both had been frozen ſome time.

HAPPENING to caſt my eyes into the fields, out of the window, through which I had put the board which I uſed for the purpoſe of this experiment, I obſerved, on each ſide of the electrified wire, the ſame *dancing vapour*, which is ſeen near the ſurface of the earth in a hot ſummer's day, or near any heated body that occaſions an exhalation of vapours.

IX. THE EXAMINATION OF A GLASS TUBE, WHICH HAD BEEN A LONG TIME CHARGED AND HERMETICALLY SEALED.

DECEMBER the 30th. I examined a glaſs tube, about three feet in length, one half of which I had charged in the month of March preceding, and then ſealed hermetically; but could not perceive that it was excited in the leaſt degree, either by heating or cooling. The difference in the reſult of this experiment from ſeveral of Mr. Canton's, related p. 276, I attribute to the thickneſs of the glaſs of my tube. Mr. Canton charged ſmall balls exceedingly thin. I alſo obſerved that there was no perceivable difference in the excitation of the charged or uncharged part of this tube, and that both parts acted exceeding well.

I AFTERWARDS opened this tube, and pouring a quantity of leaden ſhot into it, found it to contain a very good charge. It gave me one conſiderable ſhock, and ſeveral ſmall ones; as I made no uſe of an outward coating, but only diſcharged it by graſping it in ſeveral places by my hand.

X. THE WEIGHT REQUISITE TO BRING SOME BODIES INTO CONTACT ASCERTAINED BY THE ELECTRICAL EXPLOSION.

It is plain from optical experiments, and alſo from a variety of other conſiderations, that bodies of no great weight lying upon one another, are not in actual contact. As the ſame thing is demonſtrated by an electric ſpark being viſible between pieces of metal lying upon one another, and other effects of electricity (particularly the fuſion of the parts through which it goes out of one body and enters another, not actually in contact with it) I was deſirous to determine, by this criterion, what weight was ſufficient to bring bodies into actual contact. With theſe views, I began with laying twenty ſmooth ſhillings upon one another, and making the diſcharge of the battery through them; thinking that the fuſion would diſappear, when the weight was ſufficient to preſs them into contact. But I found that the whole column was not ſufficient; for every piece was melted on both its ſides, ſo that every two contiguous ſides had ſpots exactly correſponding to one another. The deepeſt impreſſions were made near the top of the column, but they did not diminiſh with exact regularity. Perhaps ſmall particles of duſt might prevent ſome of them from coming ſufficiently near one another.

Afterwards, I gradually increaſed my weights, till I found that about ſix pounds was ſufficient for my purpoſe. The fuſion was viſible under that weight, but never under above half a pound more, though I repeated the experiment ſeveral times.

I had ſome ſuſpicion, that the largeneſs of the exploſion might have occaſioned a momentary repulſion, ſeparation, and conſequent fuſion of theſe pieces of metal, though preſſed by ſuch a weight, but I found I was not able to produce any fuſion; under a greater weight than that above mentioned, though, inſtead of thirty-two ſquare feet of coated glaſs, I uſed above ſixty.

XI. THE EFFECT OF THE ELECTRICAL EXPLOSION TRANSMITTED THROUGH VARIOUS LIQUORS.

I BELIEVE it is generally ſuppoſed, that ale, and other liquors are turned ſour by lightning, and I was deſirous of aſcertaining whether that fact (if it be one) was owing to the liquors being properly ſtruck with the lightning, or to the ſtate of the air, &c. during the thunder ſtorm. In order to this, I provided myſelf with a glaſs tube, nine inches long, and about a quarter of an inch in diameter, and by inſerting a wire into one end of it, which was ſtopped with ſealing-wax, could eaſily tranſmit an electrical ſhock through any ſubſtances contained in it.

BY this means, November the 13th, I began with diſcharging the exploſion of the battery through this tube, filled with *freſh ſmall beer*, and obſerved a conſiderable quantity of fixed air, or ſomething in the form of bubbles, to aſcend in it; but when I taſted it, I could perceive no difference between it and that out of which it was taken. No doubt the eſcape of ſo much air would tend to make it grow ſtale ſomething ſooner.

I THEN diſcharged ſeveral large ſhocks through a tube filled with *red wine*, but, after two or three days, could perceive no alteration in its taſte, or other ſenſible qualities. In this diſcharge, the electric matter did not immediately ſtrike the wine, but a metal rod, which juſt touched its ſurface; but I afterwards gave it two or three more ſhocks, in which the wine itſelf was made to receive the exploſion, but there was no variation in the effects.

I passed the ſhock through a tube filled with *milk*, in both the methods above mentioned; but it was ſweet three days after. Alſo a tube filled with *freſh ale* received ſeveral large ſhocks without undergoing any ſenſible change of properties.

In all theſe exploſions I held the tube in my hand, without feeling any thing of the ſhock.

I also made the electric ſpark viſible a great number of times in a ſmall quantity of *ſyrup of violets*, without producing any change of colour, or other ſenſible qualities.

XII. Observations on the colours of electric light.

Finding it advanced in the writings of ſeveral electricians (who muſt have copied it from one another, without ever repeating the experiment, though it may be done ſo ſoon) that electric light contained no priſmatic colours; I had the curioſity to try ſo extraordinary a fact, and immediately ſaw both the fallacy of the experiment when it was firſt made, and the cauſe of it. Holding a priſm before my eyes, while the electric ſparks were taken at the prime conductor, I obſerved as beautiful priſmatic colours as any that are exhibited by the image of the ſun; but when the light was a little diffuſed, as in thoſe red or purple parts of a long ſpark, as it is called, the colours were not ſo vivid, and leſs eaſily diſtinguiſhed from one another; and when the light was ſtill more diffuſed, through a vacuum, the priſm made no ſenſible alteration in the appearance of it. Thus the middle part of any large object appears of its natural colour through a priſm: for though the rays be really ſeparated, they are immediately confounded with others from different parts of the ſame object; ſo that its natural colour muſt neceſſarily be the reſult.

As the flames of different bodies yield very different proportions of the priſmatic colours, I have often thought of attempting to aſcertain the proportion of theſe colours in electric light, and compare it with the proportion of colours from light procured in various other ways; but I have not had leiſure to purſue the inquiry.

THE electric ſpark, taken in the middle of a phial filled with inflammable air, is always of a red or purple colour, and cannot be made to look white; but the larger the exploſion is, the nearer it approaches to white.

I SHALL cloſe this article with juſt mentioning another deception, which ſome perſons may poſſibly lie under, with reſpect to what is called the *length of the electric ſpark.* When a jar is diſcharged, it may be imagined, that a body of fire is ſeen extending from the inſide to the outſide; whereas it is pretty certain, that that appearance is occaſioned by the very rapid motion of a ſingle ball of fire; in the ſame manner as a lighted torch, with no greater motion than a man's arm can give to it, will ſeem to make an entire circle of fire. That the fire of an electrical exploſion conſiſts of a ball, or cylinder, of no great length, ſeems pretty evident from one of the experiments with the circles, in which the diameter of the circle was the ſame, whether the exploſion was taken at the diſtance of half an inch, or of two inches; and alſo from the experiments of its paſſage over ſurfaces, in which it was ſometimes made twenty times longer than uſual, without any ſenſible diminution of its thickneſs.

XIII. Observations on the small wires that collect electricity from the excited globe.

Having made uſe of ſeveral braſs wires, about two inches and a half long, to collect the electric matter from my globe; I obſerved, after a month or two, that about half an inch of the ends of them, which touched the globe, had contracted a blackneſs, particularly on the ſide which lay next the globe. I then took them off the ring to which they had hung, and rubbing them carefully, obſerved, that the ſame friction which made the reſt of the wire quite bright, made but little alteration in this acquired blackneſs. Recollecting, at the ſame time, S. Beccaria's theory of magnetiſm, inſtead of replacing the wires, I hung two very fine needles in their place; and December the 20th, after about two months, in which I had made the moſt uſe of he machine, I examined them, and found that blackneſs at their points, but could not be ſure that they had acquired any degree of magnetiſm. They had, indeed, a very ſmall degree; but I had not examined them ſo very accurately before I hung them on, as I did afterwards. The experiment deſerves to be repeated with more care, but it requires a longer and more conſtant uſe of an electrical machine than, it is probable, I ſhall ever have an opportunity of employing.

XIV. Experiments intended to ascertain the difference in the conducting power of different metals.

In a converſation I once had with Dr. Franklin, Mr. Canton, and Dr. Price, I remember aſking whether it was probable that there was any difference in the conducting power of different metals; and if there was, whether it was poſſible to aſcertain that difference? I have ſince endeavoured to carry into execution a ſcheme propoſed by Dr. Franklin, viz. tranſmitting the ſame exploſion of the battery through two wires at the ſame time, of two different metals, and of the ſame thickneſs. They were hooked one to the other, and held faſt in hand viſes, after they were meaſured with a pair of compaſſes to exactly the ſame length. The experiments were much more pleaſing and ſatisfactory than I expected, but the reſult by no means correſponded to my ideas *a priori.*

I FIRST joined a piece of iron wire, and a piece of copper wire. The exploſion totally diſperſed the iron, and left the copper untouched. The braſs likewiſe diſappeared when joined with the copper, and the iron when joined with the braſs.

So far the experiments were extremely eaſy; a ſingle charge of the battery ſufficing to determine the difference between any two; but when I came to compare the more perfect metals, I found much more difficulty, and was obliged to try four or five charges of the battery upon every two: for, their conducting powers being nearly the ſame, I either made the charge too high, and diſperſed them both; or too low, and touched neither of them. At length, I happened to hit upon ſuch charges, that the copper vaniſhed, and left both the ſilver and the gold; and the gold remained when the ſilver was diſperſed. The hook, however, of the ſilver was melted off when the copper was diſperſed, and the hook of the gold when the ſilver was diſperſed: for the heat is always the greateſt where the electric fire paſſes from one body to another. Before the diſperſion both of the copper and the ſilver, I had made exploſions of ſuch a ſtrength, as though too ſmall to melt them, gave them a blueiſh tinge.

From theſe experiments it is eaſy to ſettle the order in which the metals above mentioned are to be ranked, with reſpect to the power of electricity to melt them. It is as follows. *Iron, braſs, copper, ſilver, gold.*

Not being able to get *lead* or *tin* drawn into wires, I got pieces of thoſe metals rolled into plates equally thin, and taking ſmall ſlips, of equal length and breadth, I tranſmitted the exploſion through them; when the lead gave way the firſt. I intended to have compared theſe plates with others of iron, braſs, &c. but had not an opportunity. I have little doubt but that tin would melt before iron; though indeed I had expected that tin would have melted before lead, and gold before ſilver. But according to Mr. Wilcke's experiments, lead is a worſe conductor than any of the other metals. My own experiments on the circular ſpots, made me expect that gold would have melted before ſilver.

It is very remarkable, that when iron wire is melted by the electric exploſion, *bright ſparks* are generally diſperſed about the room, in all directions; but that they are ſeldom, or never ſeen when wire of any other metal is uſed. If but a ſmall reſiduum of a battery be taken between two iron rods, when the exploſion is extremely little, a great number of ſmall ſparks will fly in all directions from the iron, to the diſtance of about an inch, and exhibit a beautiful appearance. Fewer of theſe ſparks will be ſeen if one of the rods be braſs, and, I think, none in theſe ſmall diſcharges, if they both be braſs.

Before any uſe can be made of theſe experiments, to determine the relative conducting power of the ſeveral metals, the order in which they melt with common heat ſhould be compared with the order in which they melt with the electrical exploſion; and the French tranſlation of this hiſtory places them in the following order; tin, lead, ſilver, gold, braſs, copper, iron. It is remarkable that iron ſhould require more heat to bring it into a ſtate of fuſion than any other metal, and yet ſhould require but a ſmall force of electricity to do it. Before this matter can be ſettled, it ſhould likewiſe be found, how much more eaſily any of the metals will be melted before another, by tranſmitting ſhocks through wires of different lengths and thickneſſes, which would be a very tedious buſineſs. I make no doubt but that an exploſion which melts a copper wire of any given diameter would diſperſe an iron wire of twice the diameter, ſo that copper would be a much greater ſecurity, as a conductor to guard a building from lightning than iron, beſides its being leſs liable to ruſt; but then it is more expenſive.

XV. Experiments with an ELECTRIFIED CUP.

I shall cloſe the account of my experiments with a ſmall ſet, in which, as well as in the laſt, I have little to boaſt beſides the honour of following the inſtructions of Dr. Franklin. He informed me, that he had found cork balls to be wholly unaffected by the electricity of a metal cup, within which they were held; and he deſired me to repeat and aſcertain the fact, giving me leave to make it public.

Accordingly, December the 21ſt. I electrified a tin quart veſſel, ſtanding upon a ſtool of baked wood; and obſerved, that a pair of pith balls, inſulated by being faſtened to the end of a ſtick of glaſs, and hanging intirely within the cup, ſo that no part of the threads were above the mouth of it, remained juſt where they were placed, without being in the leaſt affected by the electricity; but that, if a finger, or any conducting ſubſtance communicating with the earth, touched them, or was even preſented towards them, near the mouth of the cup, they immediately ſeparated, being attracted to the ſides; as they alſo were in raiſing them up, the moment that the threads appeared above the mouth of the cup.

If the balls had hung in the cup a conſiderable time without touching it, and they were taken out immediately after the electricity of the cup was diſcharged, they were found to have acquired no degree of electricity.

If they had touched any part of the cup, though they ſhowed no electricity while they were within it; yet, upon being taken out, they appeared to have acquired ſome; which was more if they had touched a part near the edge of the cup, leſs if they had touched any part more remote from the edge, and leaſt of all if they had touched the bottom only. If they had firſt touched the ſide near the top, and then the bottom, they came out with that ſmall degree of electricity which they would have acquired, if they had touched the bottom only.

In any caſe, if the balls were taken out while the cup remained electrified, they neceſſarily acquired ſome degree of electricity, in paſſing the mouth of the cup.

To purſue this experiment a little farther, I took a ſmall coated phial, ſuch as is repreſented upon the ſtool [*c* Pl. II.] and obſerved, that when I held it by the wire, within the electrified cup, it acquired no charge, the electricity of the cup affecting both the inſide and outſide coating alike. If the external coating touched the bottom of the cup, the phial received a very ſmall charge. If it was made to touch the ſide, it acquired a greater charge; and the nearer to the top it was held, the higher charge it received; the wire of the phial, which communicated with the inſide coating, being farther removed from the influence of the electricity of the cup.

May we not infer from this experiment, that the attraction of electricity is ſubject to the ſame laws with that of gravitation, and is therefore according to the ſquares of the diſtances; ſince it is eaſily demonſtrated, that were the earth in the form of a ſhell, a body in the inſide of it would not be attracted to one ſide more than another.

DOTH it not follow from the experiments of the balls, compared with thoſe with the phial, that no body can receive electricity in one place, unleſs an opportunity be given for its parting with it in another; at leaſt, that a quantity muſt be repelled from any particular part before any more can enter, ſince a ſmall body can no more receive electricity when all its ſides are equally expoſed to the action of an electrified body, than a phial can be charged when both its coatings are equally expoſed to the ſame electricity?

Do not theſe experiments, likewiſe, favour the hypotheſis of S. Beccaria, that there is no electrical attraction without a communication of electricity?

MR. LULLIN alſo made ſeveral of theſe experiments with an electrified hollow veſſel. He obſerved, that if an electrified cork ball, ſuſpended by a ſilken ſtring, was let down into the veſſel, and touched the bottom, it left all its electricity behind it. He alſo made the experiment with a glaſs veſſel, coated at the bottom and charged, with the ſame event. It alſo made no difference whatever was the form, or ſize of the body let down into it, provided it was one third leſs than the depth of the veſſel; but if, when it touched the bottom, it, at the ſame time, reached the top, or came near the top of the veſſel, it acquired electricity; and a conſiderable degree, if it exceeded the top. The form of the veſſel made no difference in his experiments, nor did it make any whether the veſſel was intire or perforated. A wire net anſwered perfectly well. Theſe experiments, Mr. Lullin imagines, clearly prove Nollet's doctrine of the conſtant motion of electric atmoſpheres: for he thinks, that this free motion on which electrification depends, is prevented from taking place within the veſſel, by the contrary tendency of the oppoſite ſides *.

Dr. Franklin, in the laſt edition of his Letters, p. 326, ſays, that poſſibly the mutual repulſion of the oppoſite ſides of the electrified cup, may prevent the accumulation of the electric atmoſphere upon them, and occaſion it to ſtand chiefly on the outſide. But he recommends it to the farther examination of the curious.

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* Diſſertatio phyſica, p. 38.

A CATALOGUE OF BOOKS WRITTEN ON THE SUBJECT OF ELECTRICITY, EXCLUSIVE OF PAPERS IN BOOKS OF PHILOSOPHICAL TRANSACTIONS, AND OTHER MISCELLANEOUS WORKS; DISTINGUISHING [BY ASTERISMS] THOSE WHICH THE AUTHOR HAD SEEN, AND MADE USE OF IN COMPILING THIS WORK.

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* *Rackſtrow*'s miſcellaneous obſervations, together with a collection of experiments on elecricity, 1748, London, octavo.

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Boze's poem on electricity. Hiſtoire.
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Nouvelle diſſertation ſur l' electricité par un phyſicien de Chartres. Hiſtoire.
Mr. *Kruger*'s meditations on electricity. Hall.
Tentamen de vi electrica ejuſque phenomenis, auctore *Nic. Bammucaro.*
Laurentii Beraud S. J. theoria electricitatis, Peterſburg. Wilcke, p. 12.
Watkins on electricity.

As the reader will ſee by the aſteriſms what books I have had an opportunity of peruſing, he will ſee in what parts my hiſtory is moſt likely to be defective. And I ſhall think myſelf greatly obliged to any perſon, who will favour me with the uſe of any treatiſe which contains a diſcovery of importance. But I do not apprehend that any thing very material can have eſcaped me.

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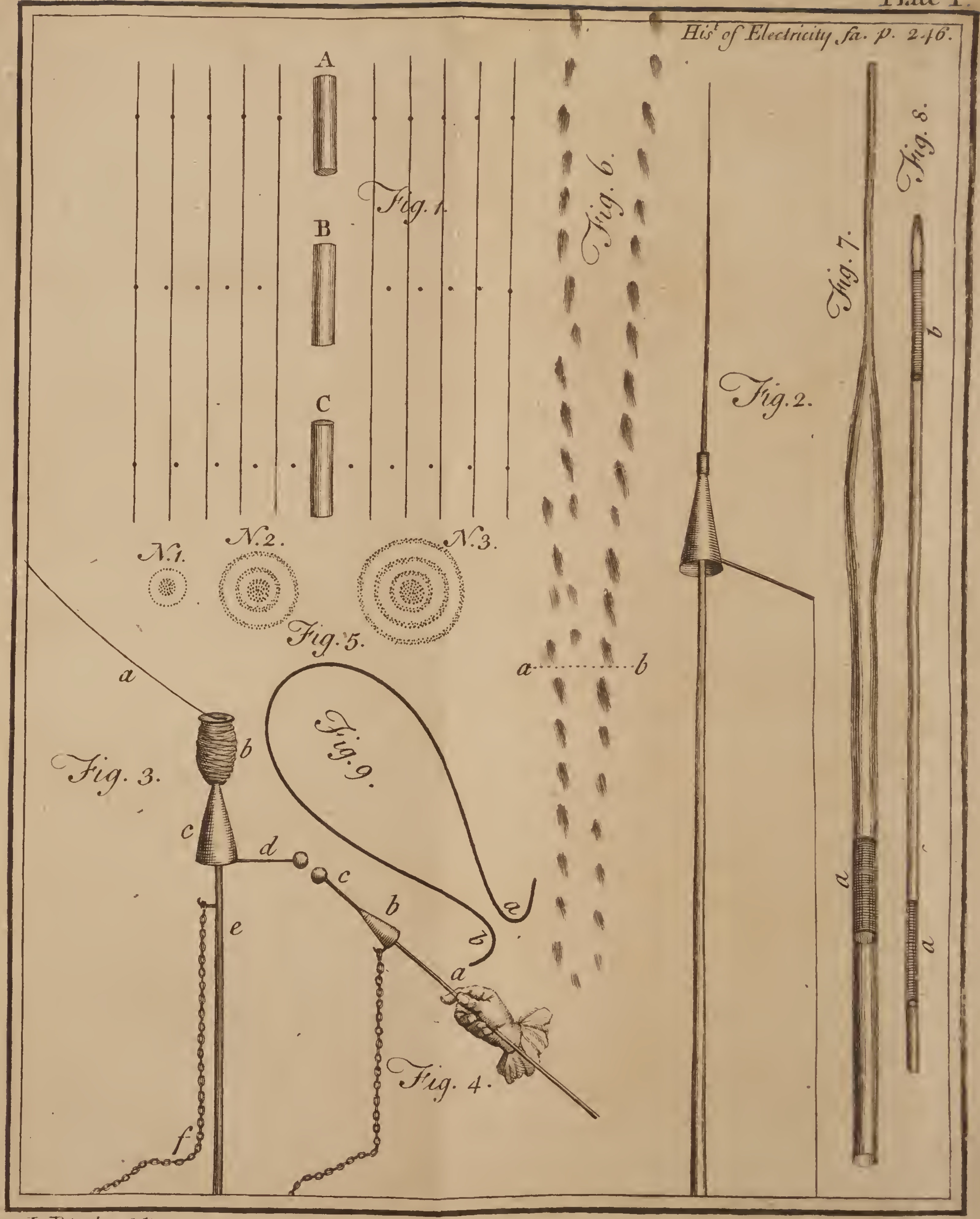

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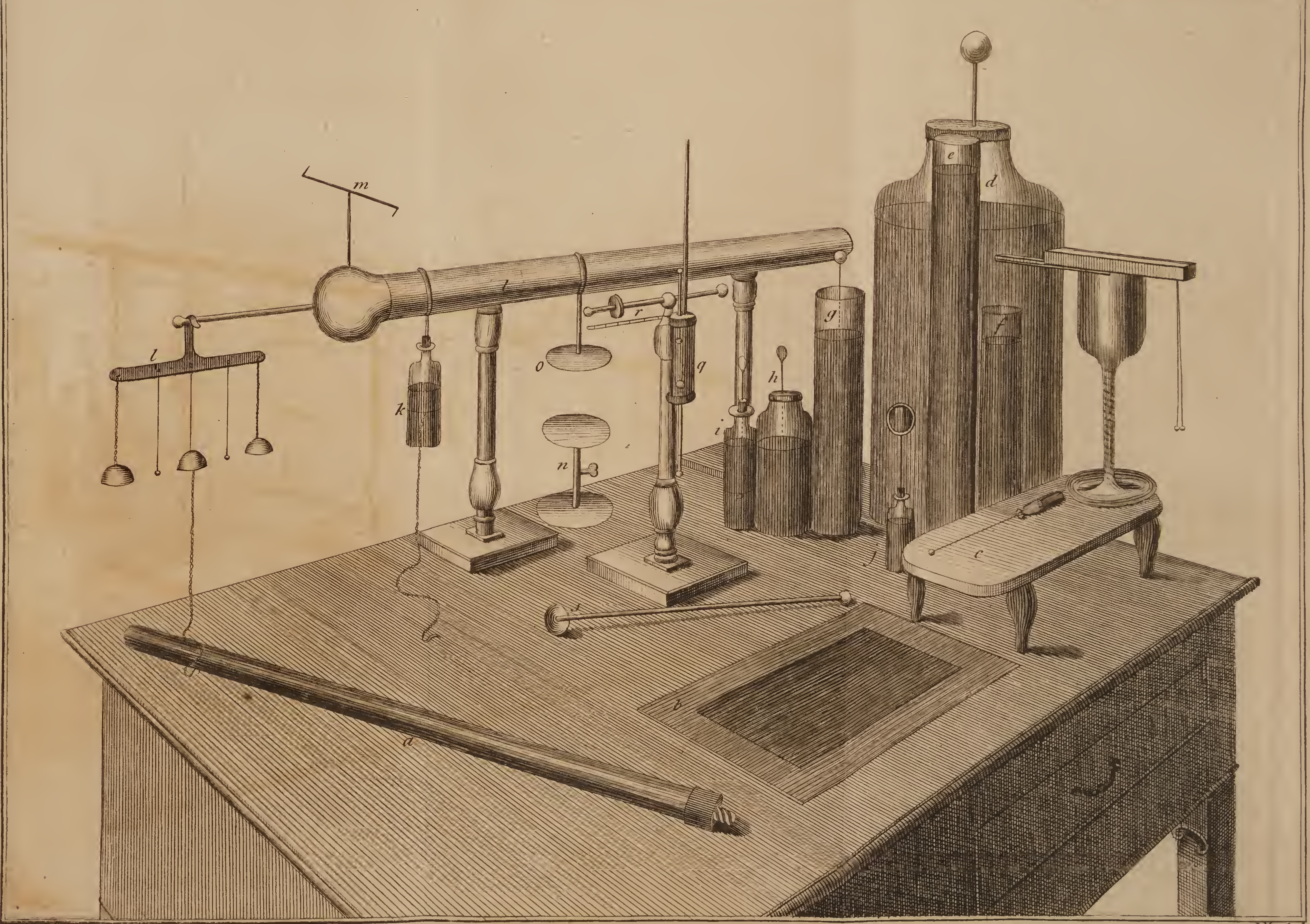

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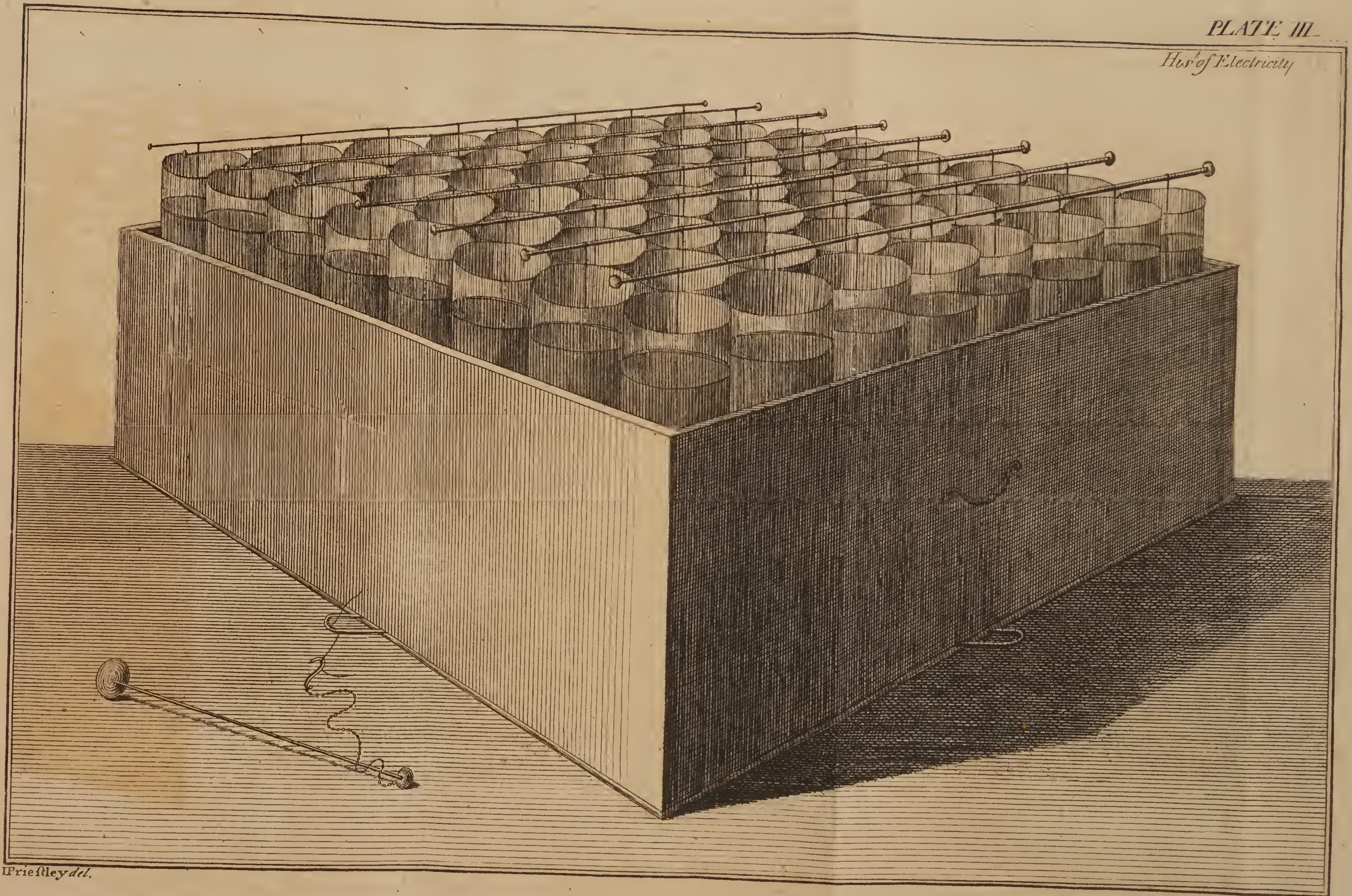

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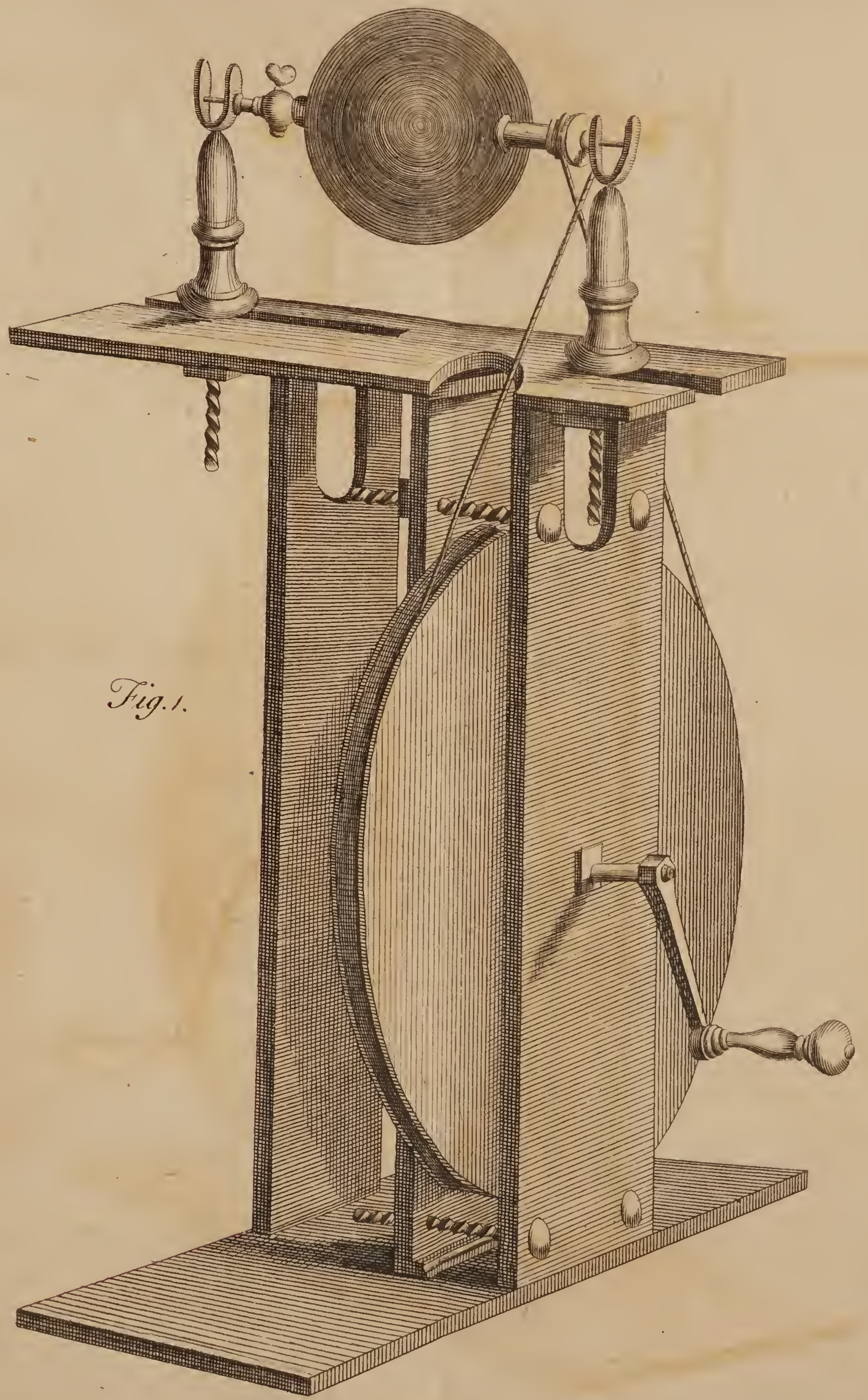

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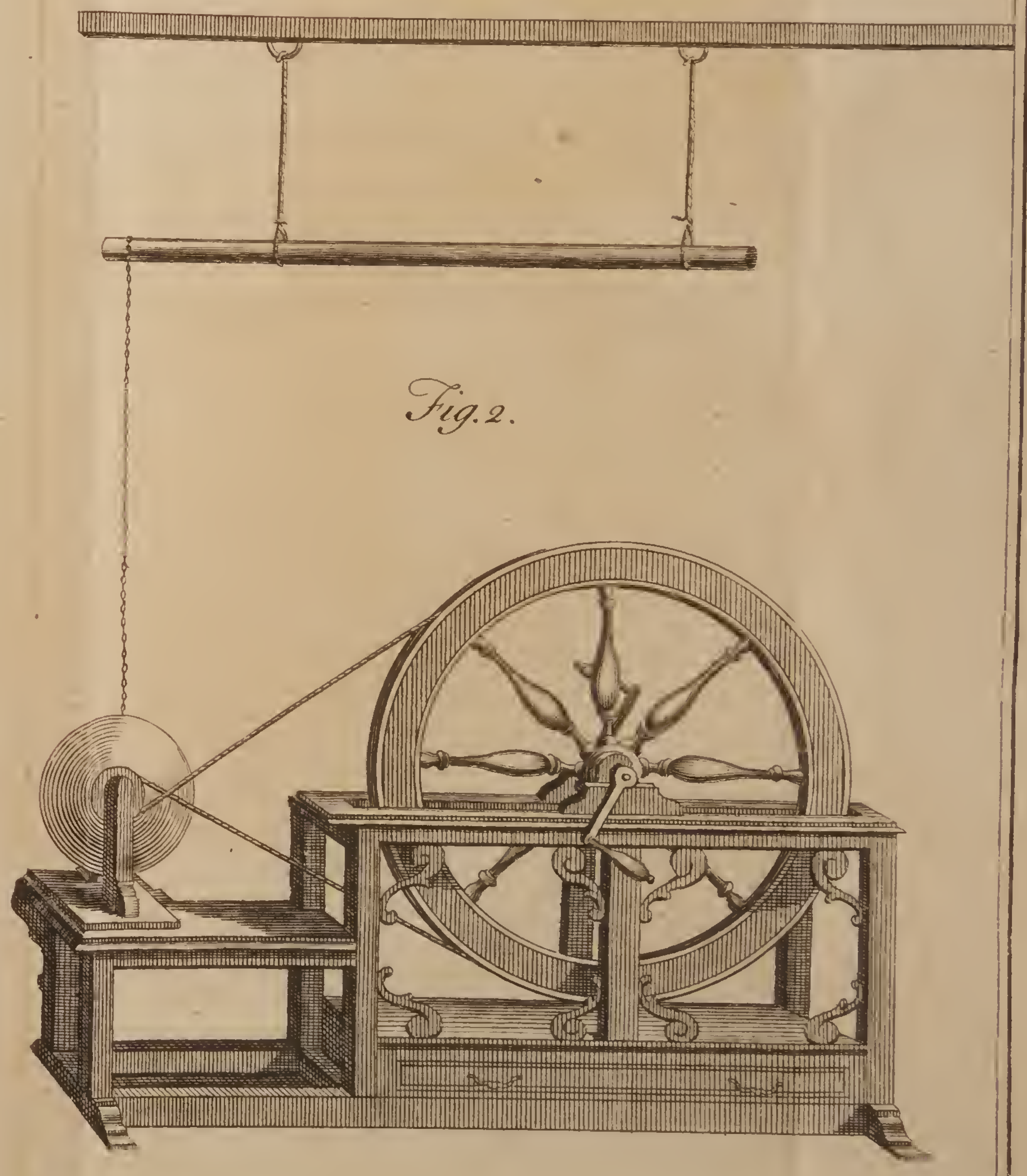

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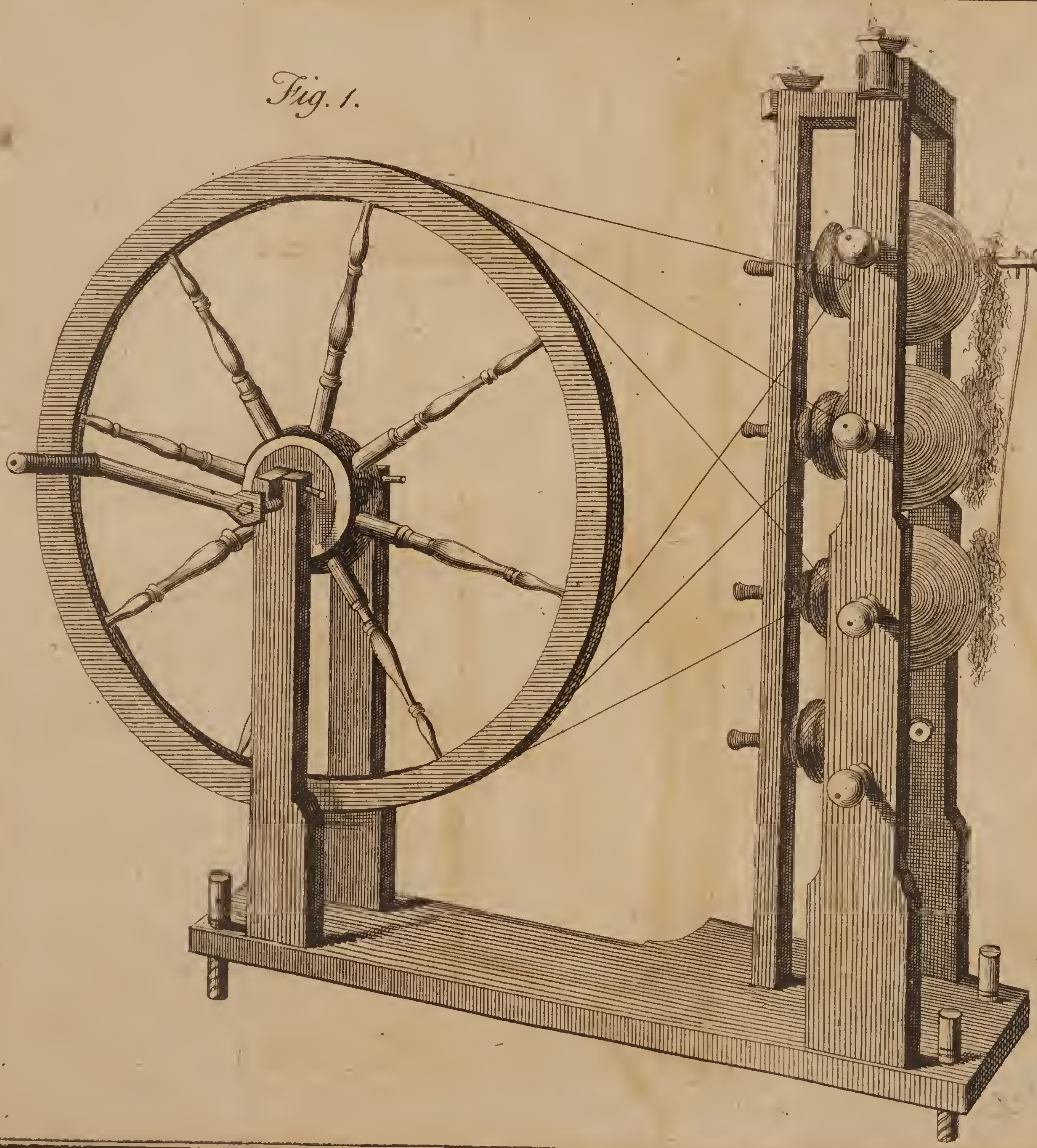

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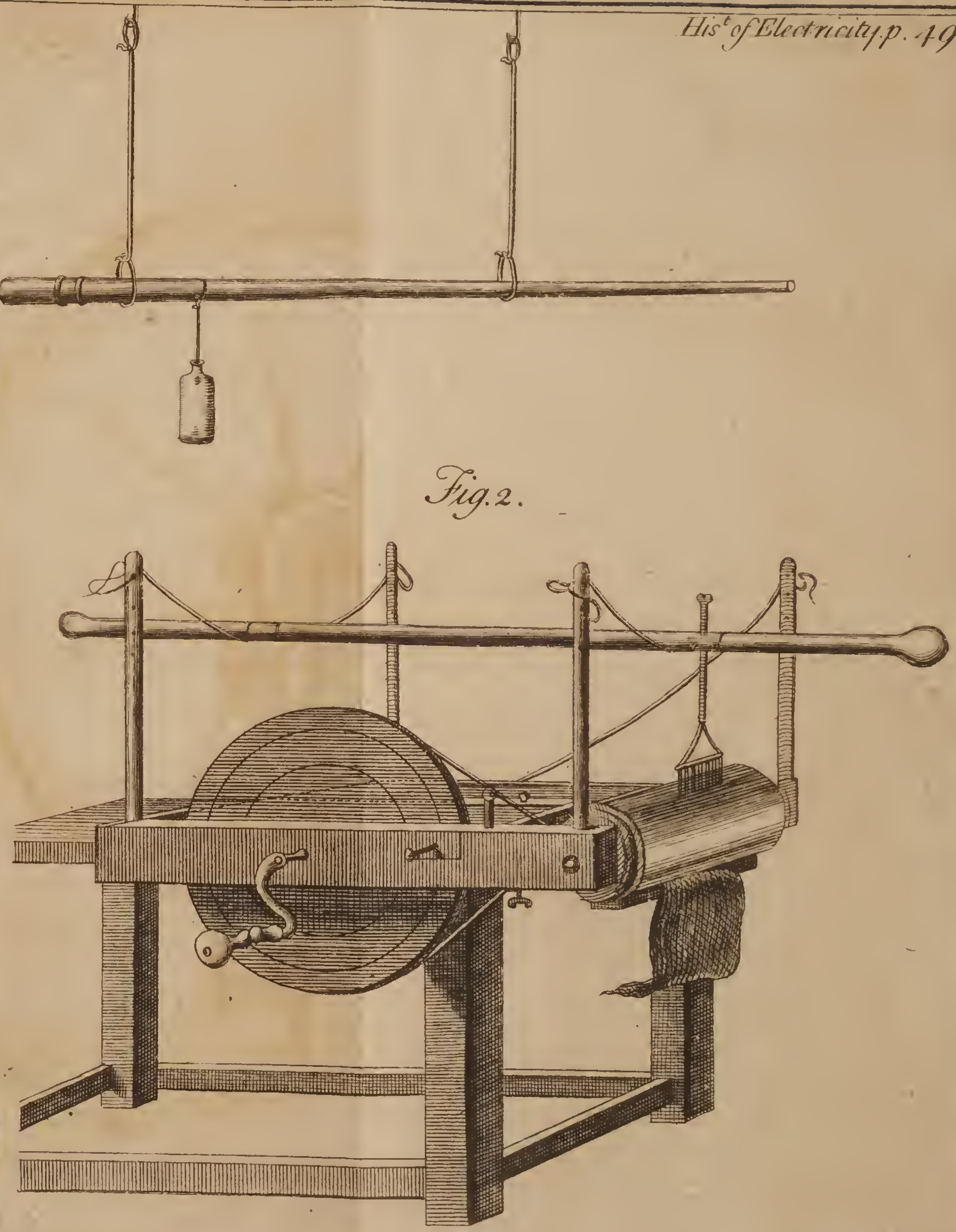

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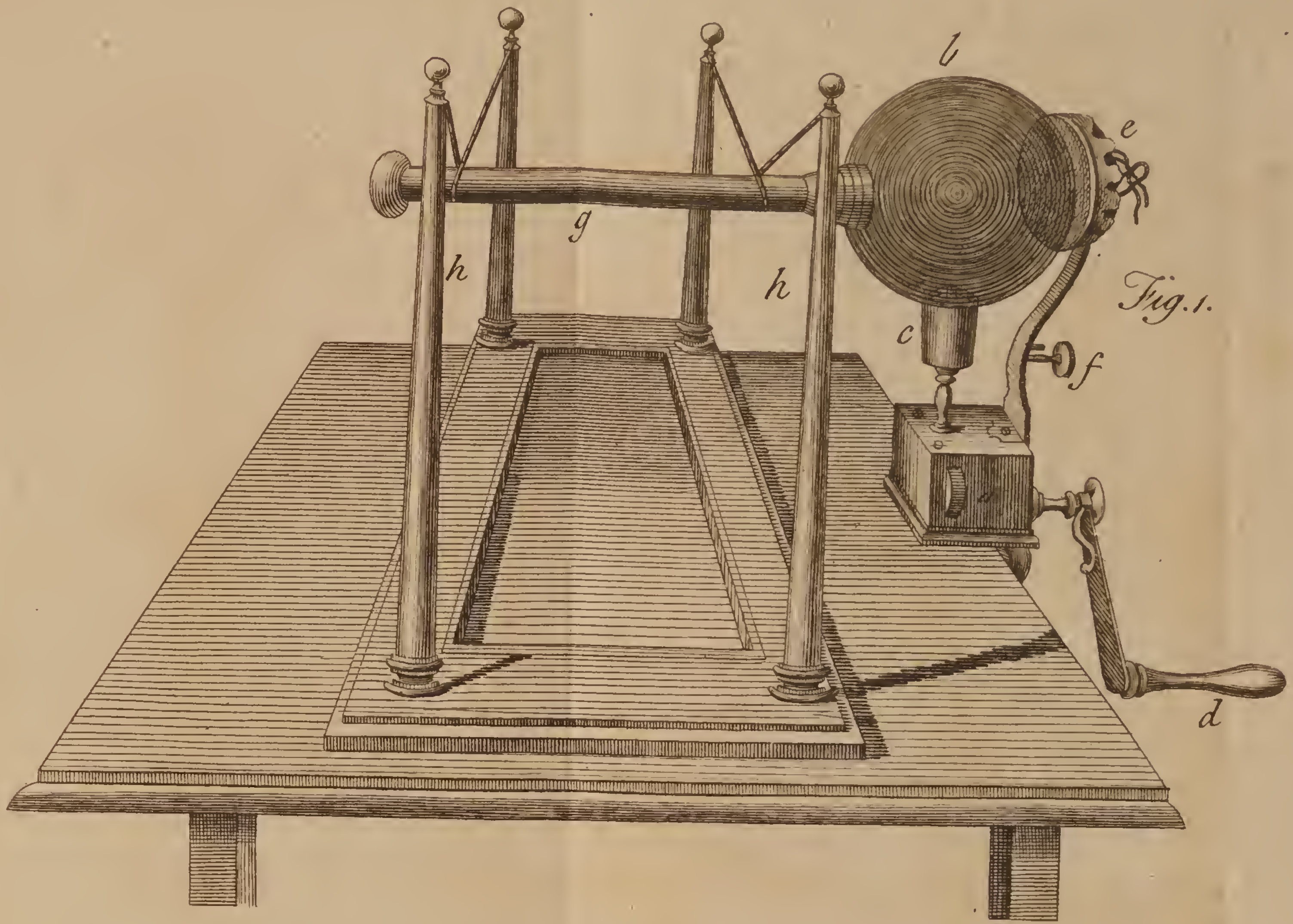
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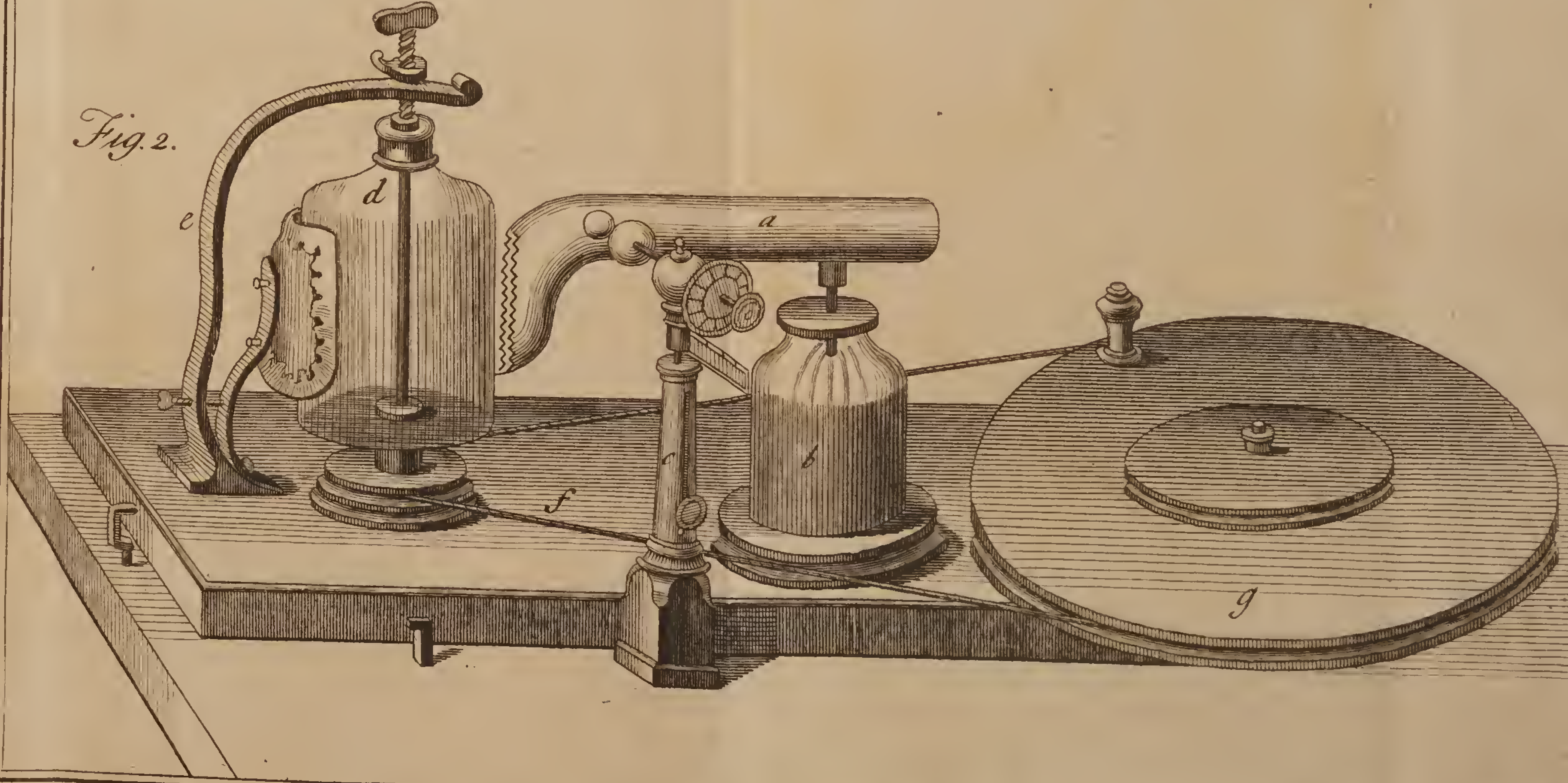
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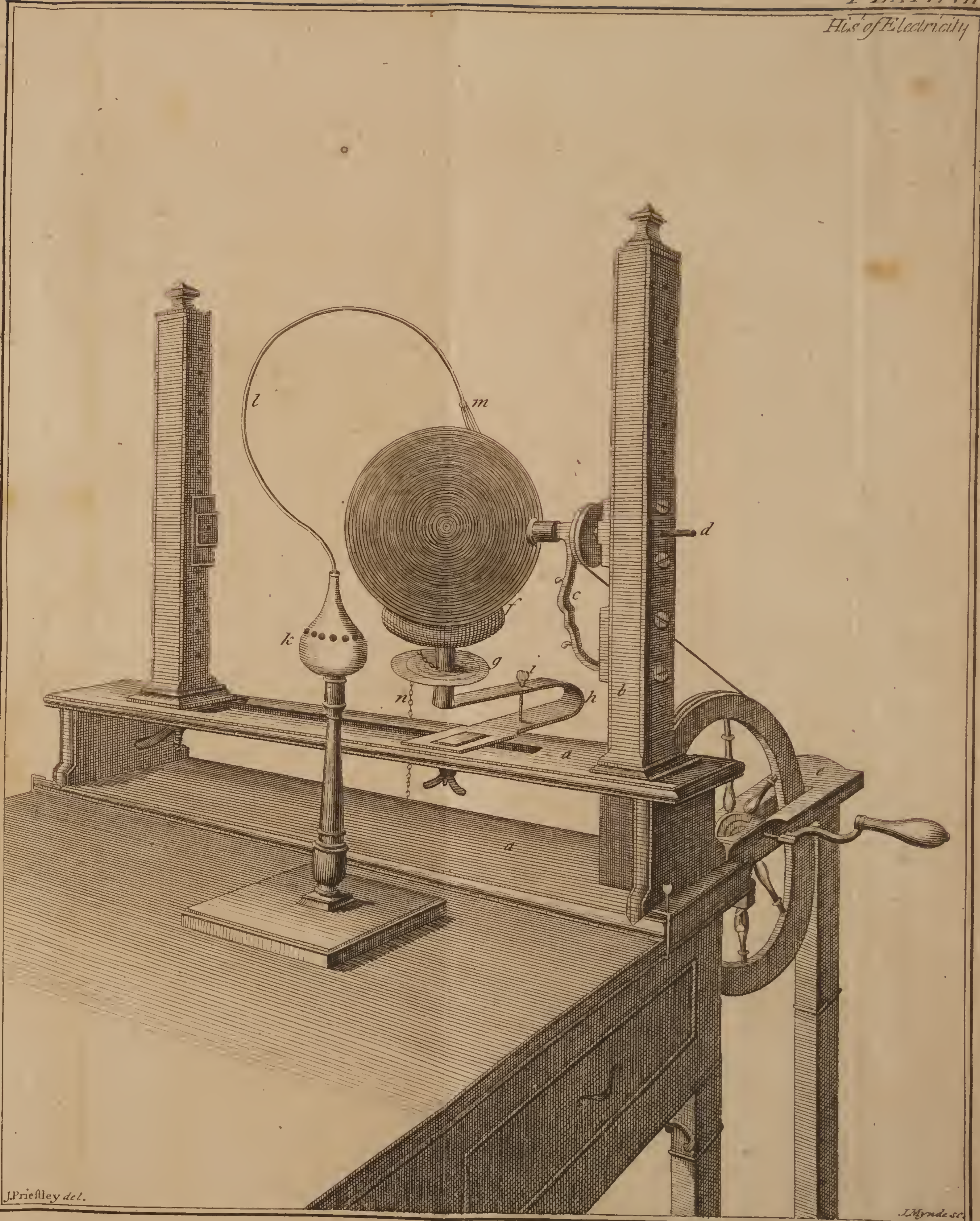

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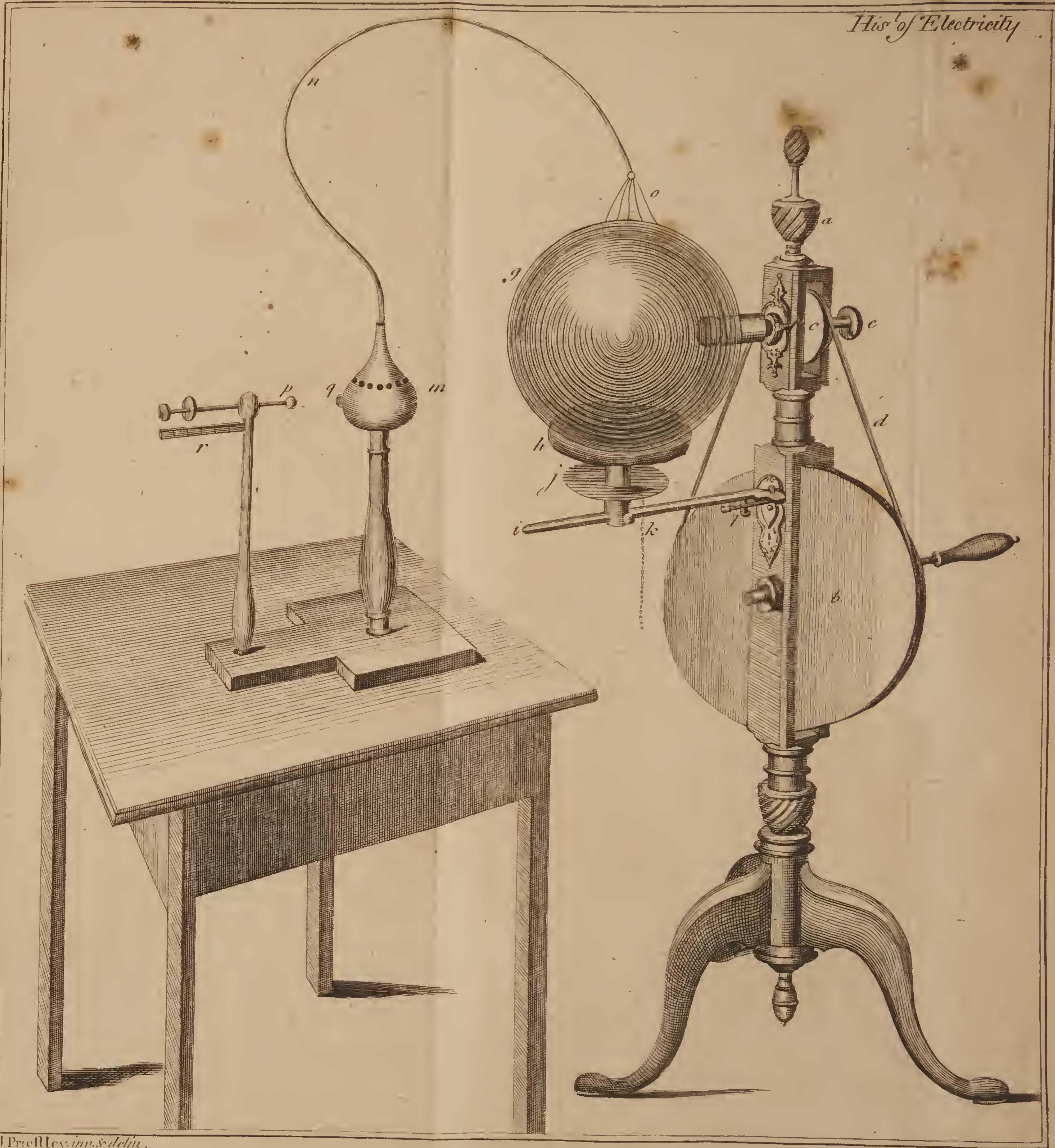

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